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Ten Lectures and Discussions on

SCIENCE: Its History, Philosophy, and Relation to Democracy

held at the Department of
Agriculture Graduate School
under the chairmanship of

M. L. Wilson
Under Secretary of Agriculture

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Lecture I

SCIENTIFIC ATTITUDES IN GREAT BRITAIN AND AMERICA

by

F. R. Moulton

The Graduate School
The Department of Agriculture
Washington
1939

LECTURE I

SCIENTIFIC ATTITUDES IN GREAT BRITAIN AND AMERICA

by

Dr. F. R. Moulton
Permanent Secretary of the American Association for the Advancement of Science

In the auditorium, at 5 o'clock Wednesday
8th March 1939

Even so simple a thing as the description of an object does not depend upon the object alone. It reflects also the mind that makes the description. A tree, for example, to the hunter is a refuge for game. To the farmer it is a protection against winds and a source of shade. The artist sees in it only a beautiful shape and a glorious color. The chemist regards it as an aggregate of complicated molecules that might easily be broken down into a few kinds of atoms. To the physicist it is a skeleton framework of electrons and protons. For the biologist it is a link in a chain of evolution that reaches back to the lowly beginnings of life. Thus each picture of a tree is in part a reflected image of the mind that describes it.

In the more complicated task of interpreting human attitudes and actions similar reflections of the interpreter's mind largely determine the form and color of his picture. Who, for example, was Napoleon and what were his motives? To many he was an inspired genius who gave a new meaning to the word glory. To others he was an ambitious and heartless madman who wasted the blood of France from Italy and Egypt to Russia and Waterloo. To others he was a crusader who carried ideas of liberty, equality, and fraternity throughout Europe. To every mind he was different, and is even to this day, although he may now be viewed in the perspective of a century of history. And so every opinion depends in part upon the object or subject considered and in part upon the person making the judgment. Therefore when I undertake to describe the recent attitudes of scientists in Great Britain and America, it should constantly be remembered that inevitably I express in part my own mind, determined as it has been by inheritance and all the experiences of life.

There is obviously no unanimous attitude of scientists at present nor has there been at any previous time. They are as different individually as their inheritances and environments have been. Yet statistically there have been and doubtless are trends in their attitudes. At first this statement may appear heterodoxical, for traditionally science is supposed to be purely objective, to be the pursuit of truth for its own sake. Yet it has not been created by intellects that were independent of their environments. Science is the product of imperfect and biased human minds and partakes of their imperfections, especially in its interpretations and applications. Consequently, the problem of determining scientific attitudes

is one that is incapable of solution in any exact sense.

For present purposes it will be convenient to divide the history of science into three periods, the first extending from its ancient beginnings down to the World War, the second covering the decade following the World War, and the third the past ten years of depressions and recessions. There has been a somewhat different general attitude of scientists toward science and the problems of mankind during each of these periods. And reciprocally, in each of these periods there has been a somewhat different attitude of the world toward science. In order to set forth the present period in clear relief a few comments on the earlier ones will be advisable.

It has recently been maintained, particularly by Hogben, that science has grown up in response to definitely recognized needs. These needs have been described in terms of the work of the world--the problems of providing food and shelter, of traveling from one place to another, of moving heavy bodies, of defense against enemies. That such needs have in many instances led to experimentation and invention can not be doubted. But that they have been the primary motives in the fundamental advances in science is questionable. As may be illustrated by innumerable special examples, the real urge to discovery has often been in the restless and inquiring mind of man, as it has been also in the creation of philosophies and theologies. It is certainly true that most of the great discoveries in science long lacked applications to the practical affairs of life. Lest the example of geometry be cited in opposition to these statements, I promptly admit that some of its propositions were undoubtedly useful in determining the boundaries of overflowed land after recessions of the Nile; but at the same time I insist that most of the propositions of geometry were never used in such simple problems, as indeed they are not at this day. The work of Euclid was crested primarily to meet imperative demands of the human mind. For the same reason the Greeks investigated and described the beautiful properties of the conic sections, which had no practical applications until the time of Kepler nearly two thousand years later.

One of the great milestones in the progress of science was the development and the acceptance of the heliocentric theory of the solar system. Copernicus did not spend many years of laborious computations on the motions of the planets in response to any needs of the world about him. Instead, in order to avoid trouble he withheld the publication of his manuscript until the year of his death. Galileo did not maintain that the earth revolves around the sun for any practical purpose. On the contrary, he was branded as a heretic and compelled in his old age to recant and to deny the truth of his teachings. Although the correctness or falsity of the heliocentric theory had no bearing on any practical problem, the work of Copernicus and Galileo changed the whole world. Man was taken from his proud position at the center of the universe and placed on a relatively small satellite of the sun. It was not until after a bitter struggle continuing for more than a generation that he finally realized and admitted how humble is his abode. And he was equally slow in realizing that in this change he had risen to an eminence of understanding that much more than

compensated him for the relative insignificance of the earth on which he lives.

Every great advance in science has been due primarily to a curiosity that would not be denied. It was so in the case of Newton's discovery of the law of gravitation and Laplace's theory of the motions of the heavenly bodies and Dalton's atomic law and the law of the conservation of energy and Hutton's history of the earth and Darwin's organic evolution. It is so to a large extent at the present time. Otherwise why should six million dollars be expended to construct a larger telescope than any now in existence?

Down to the time of the World War science was cultivated primarily for the purpose of learning more about the universe of which we are a part. In this period one seldom finds in the reports of scientific meetings references to applications of new discoveries to practical affairs. More often a scientist boasted that his work was useless. In the beginning of the twentieth century the sciences were subdivided more and more into narrowly restricted fields. Those became the days of specialization and of "learning more and more about less and less." It is with no disrespect that I speak of them in this manner. The amateur spirit prevailed--the making of discoveries for the sheer joy of doing it. Eagerly men learned of X-rays without asking whether they might be useful; of the disintegration of radium without caring what might be done with its radiations; of chromosomes and genes simply that they might understand how a stream of life maintains its identity; of the cosmic rays, interesting because they were new messengers from inaccessible depths of space. Like schoolgirls joyously scattering garlands of flowers on a May day, scientists bestowed their rich treasures with prodigal hands on one another and on all mankind.

Then the World War came. The applications of science were quickly turned to destroying property, to taking human lives, to striking in the dark, to starving the defenceless, to the production of chaos. In this horrible work many scientists played distinguished roles. Eventually human endurance was exhausted and the war closed. Then men and women turned to pick up again the threads of their lives, but found themselves in a different world. It was not simply a somewhat different world. It was not simply different politically, for that alone would not be important. There were broken homes and wrecked lives and international hatreds and vain longings for those who would not return. Although there had been innumerable examples, in homes as well as at the front, of bravery and fortitude and a generous spirit, still something fine had gone out of the world. And the attitude of science, too, had been substantially changed.

During the decade following the World War the amateur spirit of science on the whole declined. Might was placed on a pedestal--not indeed the might of war, for the world was weary of fighting, but the might of mass production and accumulation of wealth. It was universally recognized that only science and its applications could provide the means for an enormously increased production. Therefore, science was exalted for utilitarian reasons; the scientist became a minor hero; the position of science in the universities was raised, and industries poured money into

research laboratories. A glorious millenium, created by science, seemed about to arrive. If ever in history science was cultivated for utilitarian purposes, it was in this period, especially in the United States.

There have been important differences between the attitudes of scientists in Great Britain and this country in the decade that followed the World War. Great Britain emerged from the war exhausted, and she passed through a period of stagnant industry and severe social unrest comparable to that which this country has experienced during the past six or eight years. Consequently she did not have a silk shirt era nor one of exuberant production and speculation. There was more time and disposition for reflecting on values. Attitudes toward science and philosophical speculations respecting its nature often appeared and seemed strange to Americans. In this period suggestions were made that science be halted for a time until the world should catch up with it. Now, since a depression has settled down upon this country like a cold fog, the same suggestion is heard here. There appeared sometimes during these years a mysticism in the science of England, perhaps for the first time in its history, a tendency to find reality only in the mind. One writer even went so far as to infer from our pitifully limited knowledge of the physical universe the nature of the mind of God and to suggest that it is mathematical.

Even if American scientists had not a little later claimed to base certain theological doctrines and the freedom of the will upon the "uncertainty principle," we ought not to be surprised at this period in the science of Great Britain, for she suffered much during the war. Nearly every British family mourned the loss of a relative; the burdens of caring for her maimed and broken were heavy; and the future contained no bright promises. Under these conditions it is not to be wondered at that one eminent scientist, grieving for a son lost in the war, should have been confident of being able to communicate with the spirit world. Thus during the decade following the war it was widely believed, especially in the United States, that science would provide easily and abundantly for all the physical wants of mankind, and both in Great Britain and this country that it would furnish a new and sound basis for a theology differing not greatly from that which had previously been accepted.

Confidence that science would save the world in these simple ways was soon shattered. The disillusionment was distressing. That which appeared to be a solid foundation had failed. Since realizing the situation we have been frantically grasping, like a drowning person, for help from everything within our reach whether a life preserver or a stone. We hear that science causes unemployment and hunger and want, that it makes the rich richer and the poor poorer, that it destroys religion and promotes international strife; and on the other hand, we hear that we suffer because of lack of science in business and government, and that scientists should run the world for a change.

All of these statements are rather naive and most of them have often been refuted. There is probably a lack of science, or at least of

methods characteristic of science, in government, for example. But that scientists could promptly improve on the management of business or the carrying of governmental responsibilities is a more than doubtful assumption. Scientists have the same general inheritances as other men, their experiences of childhood are similar, they take the same courses in the secondary schools and most of the same courses in the colleges, they have the same various political and religious beliefs, they have similar hobbies and recreations, they are in every way essentially the same except in their specialities. What reason, then, is there for supposing that scientists are particularly qualified to manage the world? No more reason than to assume that a botanist would do better than an engineer as the manager of a manufactory.

Fortunately it is being realized that science is neither the principal cause of the ills of mankind nor a simple cure for them. Science has had an enormous influence in the world. On the one hand, it saves human life; on the other, it provides new means for its destruction. On the one hand, it increases the productivity of the individual; on the other, it makes him more dependent on his neighbors. On the one hand, it removes superstitions; on the other, it raises new fears. Every advantage it provides is partially offset by some new problem it raises. Every direct disadvantage is at least partially compensated for by some beneficial effect. Science permeates every human activity and influences every attitude. Naturally any simple statement about its effects and how it should be directed is imperfect.

Within the past few years great interest has arisen concerning interrelations of science and society. The first comprehensive statement of the problem was by Lord Stamp in his presidential address of the British Association for the Advancement of Science in 1936. His approach was not that of the narrow specialist in science, nor that of a man who regards science simply as a means of accomplishing more effectively something in which he is interested. With a background of an immense experience in public life and in the directing of great economic forces, and with a wide acquaintance with science as well, he set forth the complexities of the political, economic, social, intellectual, and emotional problems of modern life. Instead of suggesting any ready panacea for present ills, he advocated thorough and prolonged investigations of the interrelations of all the forces, including science, that are important for the civilization of our day.

The approach of Lord Stamp to the problems of science and society is eminently sound. It is, indeed, the scientific approach the method of which is first to establish a sound factual basis and then proceed to generalization and conclusions. Within a few months after the address of Lord Stamp a series of conferences on science and society was independently planned by the American Association for the Advancement of Science, under the direction of Dr. Harold G. Moulton, President of the Brookings Institution of Washington. These conferences, two of which have been presented at meetings of the Association, were planned to develop systematically all the principal interrelations of science and society. The first one on fundamental resources as affected by science covered the enormous increase in usable natural resources due to science. An illustration is the inexhaustible supply of nitrates that can now be produced from the nitrogen

in the air. The second was on world standards of living as they exist today and as they promise to be in the future as a consequence of science. The third, to be presented in June, will be on the interrelations of science and the machinery of the economic system. The fourth will be on the interrelations of science and government, and the fifth will be on science and human beings. Thus the climax of the series will be on man himself. In the course of a long time, of the order of a million years, man has risen from the level of the higher anthropoid apes. Now a new and more potent influence than any in the past has suddenly become a factor in his evolution. The question of its long-time effects on the individual man as a thinking, moral being is perhaps the most interesting one that has ever been raised. Its answer depends on all the great principles of science, such as the theory of evolution, and it involves, too, all the past of society and all the future of everything in the world.

There is so much in common between British and American science that a few months ago reciprocal relationships, involving an exchange of eminent lecturers on alternate years, were entered into between the British Association and the American Association, and it is hoped that similar relationships will be established with other scientists throughout the world. But these methods are too slow for many ardent spirits. With the world apparently about to burst into flames they can not wait. They hope to save civilization by prompt action. To provide them with adequate machinery, the British Association at its meeting last August organized a section on the social and international relations of science. Under the organization of the American Association such a new division is not necessary. In both countries other groups are being formed, their members often being fired with the zeal of crusaders. Though they may be disappointed at the slow improvement of society, their motives and efforts are highly praiseworthy. They will at least force on the attention of the world the fact that science and its applications have become suddenly perhaps the most powerful influences affecting the future of civilization. The mere realization of this fact will stimulate reflections on the question of human destiny, not in the minds of philosophers alone but also widely in the minds of common men. These reflections will lead to conceptions such as not even philosophers in earlier periods have held, for they will be harmonious with the background furnished by science--a lawful universe, the long geological ages since the earth was young, the slow evolution of life and intelligence, the prospects for an indefinite future of the earth as a place suitable for the abode of man.

But in the meantime civilization appears to totter on the brink, though probably its recuperative powers are enormously greater than pessimists suppose. Certainly it has risen far in the past hundred thousand years, and certainly it falls very far short of ideals we can easily formulate. The problem is to determine what we should do to save what is best in the past and to achieve what is most promising for the future.

It may appear to be an anticlimax to turn our attention for a moment from world problems and a feeling of personal responsibility for

civilization and think simply about things which are more nearly under our control as individuals. If any considerable fraction of men should cease attempting to tell others what to do, and, instead, should search for defects in their own minds and hearts, the future of society would immediately be brighter. But this is not the way the human mind normally works. It desires to make the world conform to its own pattern. For this reason it is supremely important that there should be developed in the backgrounds of the minds of the masses of men a broad general philosophy that will be beneficial both to individuals and to society. In the development of such a philosophy science can perhaps make its greatest contribution to the progress of civilization.

As a matter of fact, science is based upon a principle of such profound significance that if it should become firmly established in the intellectual constitution of the masses of men the world would be transformed. That principle is simply that the universe is orderly. This statement means that there are repetitions among the constituent units, such as atoms, of which it is composed and that natural phenomena succeed one another in orderly sequences. A less cautious and perhaps more naive statement of the principle is that the "law of cause and effect" is always fulfilled. In order not to imply any control or compulsion by an unknown power, we shall retain the more explicit statement of the principle, a very condensed form of which is simply that the universe is orderly.

Every scientist will readily agree to the statement that the universe is orderly, and so will most other persons. But do such acquiescenses mean that the principle is really an essential part of the intellectual constitutions of those who make them? Or are they somewhat thoughtless assents to an oft-repeated formula? The latter is undoubtedly generally the case. The conception that the universe is capricious probably had its origin in the apparently capricious actions of many of our fellow men. In their lives they apparently exhibit the power of arbitrary decisions that we feel strongly we also possess. But this subconscious feeling that the universe is not completely orderly does not rest alone upon our experiences as individuals. It has been bred in us in the lives and the beliefs of thousands of generations of our ancestors. It is crystallized in the earliest legends and myths that have come down to us, according to which the heavens above, and the mountains and the sea are peopled by captious gods and goddesses who must be humored and often appeased. Not only our folklore but also our literatures are largely made up of it. Read, if you will, Milton or Shakespeare or the sacred writings of any theology, with the supernatural and the miraculous on every page. Listen to the fairy tales that are told to our children, to the stories of ogres and goblins and witches that poison their young minds. If a voice is raised in mild protest against such misdirections of budding intelligence, it is suppressed and stilled by specious defenses of the falsehoods that are told them. More than half of the education in our schools and colleges accepts as normal and true things that are directly contrary to the very basis of science. It is not strange, therefore, that even the scientist is often wholly unscientific except in his own specialty, and it could not be expected

that the mass of men should have an abiding faith in the invariable orderliness of the universe.

This subject that we are considering is not one simply of idle curiosity; it is the very basis of intellecual integrity. A mind with an unwavering confidence in the orderliness of the universe is a stranger to superstitions and is harrassed by no fears. It places no trust in magic and does not believe it has a lucky star. It is not harrassed by doubts and uncertainties, for it does not sail on an unchartered sea. It serenely accepts life and rejoices that there is no chaos with which it must contend.

Up to the present time science has had its most important effects upon the physical aspects of our lives. It is true that it furnishes us more leisure, mostly to do more of the things we had previously been doing; it makes available more books to read, but largely of the kind we had read before; it tells us stories of new wonders, but too often as having been discovered by some sort of magic; it represents scientists as being wizards and magicians rather than as shining examples of completely honest minds; and it sometimes claims that it can be made the basis for such non-scientific things as theological doctrines. The first acute need is for scientists themselves to become strictly scientific, not only in their special fields but also in their entire lives. They who are thorough and cautious where they are experts should not be rash and dogmatic where they are amateurs. The second is to instil the essence of the basis of science deeply into the minds of the masses. This is no easy task, but when it shall be accomplished in some distant future, the human race will approach the millenium. I fear that until the essential basis of science, that is, the orderliness of the universe, shall become an ever-present part of the intellectual lives of men, the fears and uncertainties that now disturb them will not completely pass away.

In recent years there has been a substantial extension of the foundations of science. The orderliness heretofore considered has pertained to each individual unit, such as a planet in the solar system or a molecule in a gas, even though its behavior depended upon many other units. Thus the motion of each planet can be predicted though it depends upon the attractions of all the other planets, and the motion of each molecule in a gas depends similarly upon its encounters with other molecules.

There is, however, an orderliness in great aggregates of similar units that is simpler than the orderliness that pertains to any one of them alone and is often much more important. Let us at once consider a classical example of it, the problem which perhaps more than any other directed attention to this order in aggregates. Every one knows that a gas, such as the atmosphere in a room, is composed of innumerable molecules. It is impossible to determine where any particular molecule is at any time and at what velocity it is moving. Even if its position and velocity at any instant were given us, we could not follow it in the future because of the mathematical complexities. Therefore in a sense

the molecules in a room are a perfect example of chaos, the future of every one of them being wholly unpredictable. In order to make clear the completeness of this chaos, let us assume that the motions of our earth and of the larger things on it are also not only indeterminate but what would appear to us haphazard. Then we should never know but that the earth might some day plunge directly toward the sun or shoot off into the night of space. A river might start to flow from the sea up toward the mountains, a brick dropped from a window might dart up into the clouds. Of course, we can not really imagine any of these things happening because we understand the orderliness that is in the motions of the earth and the larger things upon its surface. But the erratic behavior that has been assumed is not greatly different from that which exists in the molecules of a gas. Nor is it essentially different from the chaos that is tacitly and vaguely accepted by those who place trust in any kind of luck or magic.

Yet a gas has many properties that are as determinate as those of a brick. These properties depend upon its total mass and volume, the masses and numbers of its different kinds of molecules, the energy which it contains, and the ways in which it gains or loses energy. Let me enumerate some of its determinate properties. Its total mass remains unchanged; the kinds and proportions of its molecules remain constant, except of course when there is chemical action; the average of the squares of the velocities of its molecules is proportional to its temperature and can be determined (about 1500 feet per second at the surface of the earth at 70°); the average of the squares of the velocities of its different kinds of molecules are inversely proportional to their molecular weights; the pressure exerted by the gas as a whole and by each of its components separately is definite and determinable; and the changes in its temperature when it is compressed or permitted to expand can be computed. If our interest were limited to these properties of a gas it would appear to us as orderly as a planet.

A strange thing has happened. We have found order in what appeared to us to be a chaos. This phenomenon is not exceptional. On the contrary, it is very common, perhaps universal. It appears in vital statistics and is the basis for mortality tables. It is seen in the rainfall for a continent, the chemical balance in our blood, the properties of atoms as described by the new statistical mechanics. It plays as conspicuous a role in certain important fields of science as the laws of motion and the law of gravitation did in another field in the days of Newton.

The fact that order has been obtained in some cases out of what appeared to be a chaos has often led to misconceptions concerning the nature of the phenomenon. There is an almost irresistable tendency to express the results as an average and to apply them to an individual case. For example, a man at seventy years of age counts rather confidently on a certain number of years of life because of the figures given in mortality tables. The results, however, do not pertain to individual cases, but apply to aggregates as wholes. When this fundamental fact is ignored errors may be committed. In recent years we have

heard much of the "uncertainty principle" and of repeated suggestions that it lays a foundation in physical science for the freedom of the will. Such suggestions are based on misconceptions of the nature of the process we are considering, and on misconceptions also, I think of the real nature of the "uncertainty principle." Last summer at the meeting of the British Association for the Advancement of Science, Dr. C. G. Darwin described it briefly and accurately when he said that it is a statement that does not pertain so much to the future of a particle as to uncertainty regarding its present position and motion. Such sweeping conclusions as the freedom of the will can not be legitimately drawn from the uncertainty principle. To attempt to do so is to abandon science. There is no reason to believe that something can be obtained out of nothing, even by averaging.

However, I must not let this digression divert our attention from what is most important in the matter under discussion. There is a relatively new science of aggregates which has techniques all its own. It appears at the atomic and subatomic levels in physics. It is found in problems relating to the equilibria of solutions of various substances. It is involved in all questions concerning populations and economics. It is basic even in discussions of the evolutions of systems of stars. The foundations of this new science of aggregates rest in part upon what is known as the theory of probability. This term unfortunately suggests the word "chance," and on this word ride all the fond superstitions we have inherited and acquired. But this new aspect of science opens no door to a chaos. Instead, it adds to the rather severe orderliness of individual units the richer order there is in aggregates.

On the whole, science in the past has been analytical, but now its snytheses are rapidly becoming more and more important. As illustrations, a living cell is much more than the sum of its molecules, just as a word is more than the sum of its letters; a man is much more than the sum of his cells; and society is more than the sum of the human beings of which it is composed. A living cell depends upon its molecules, and these unstable organic molecules depend upon the continued life of the cell. Obviously a man depends upon his cells and they depend upon him. If he has in his body the wild cells of carcinoma, they destroy him; if he dies, all of his cells die. And it is similar with society and the individual human beings of which it is composed. Since they are mutually dependent there is no particular point in raising the question whether individuals exist for the state, or whether the state exists for individuals. They have different and non-competing functions, but each depends essentially upon the other. What is immediately advantageous for one may or may not be advantageous for the other, for very complex reciprocal relations are involved. This is one reason why opinions respecting governmental policies so often disagree.

Science has proved that the universe is very complex. Even an atom is a highly organized compound unit whose structure and properties are only beginning to be understood. As yet we have only rough sketches of living cells, while the complexities of all higher forms of life are beyond imagination. There is no probability, there-

fore, that any magic formula for curing the ills of society will ever be invented. It will not be invented in England or the United States or anywhere else in the world. This outlook would be discouraging if it were not for one thing, and that thing is the orderliness of the universe. The relatively simple order that prevails in the fields of inorganic chemistry and physics will make possible undreamed-of discoveries and technological applications. As the more complicated order in the organic world gradually becomes better understood living organisms will probably be designed and constructed as organic molecules are at present. When we come to the question of human beings and human society we hesitate, overcome with emotion at the significance of the problem, as Newton hesitated before completing the computations that verified his theory that the earth's gravitation controls the moon. Newton found that the motions of the moon are orderly, though enormously complex. We are confident that the universes of human minds and human society are orderly in the new statistical sense. To quote the title assigned me for this address, this is "the most fundamental type of thinking among present day British and American scientists." It is fundamental because it guarantees us that there is a sure road, though it be long and difficult, to levels for human beings and civilization that are at present inconceivable. Let us believe this with all our hearts and live it every moment of our lives and teach it to our fellow men.

SEMINAR

following the lecture by Dr. Moulton
Office of the Under Secretary
Thursday morning at 9 o'clock, 9th March 1939

Syllabus

1. Where is science going, or is it going where we direct it? How can it be directed? P. 13.

2. What is the origin of the urge to pursue a scientific investigation?

    a. Social needs--Tuckerman, p. 14; see also the reference to Hogben on p. 2.

    b. Curiosity--Moulton, pp. 15, 23; see also the lecture, pp. 2-4.

    c. Both--Seeger, pp. 16-17.

3. How can a scientist be a useful influence in the community in matters that require the specialized training that he can furnish? Pp. 14-17.

4. Is science an aggregate of facts, or is it a method? P. 16.

5. One of the most useful things that scientists can do is to develop complete intellectual honesty in all matters that they deal with. P. 18; see also the lecture, p. 8.

6. What is the most fundamental postulate of modern science? The universe is orderly. Pp. 18, 22; see also the lecture, pp. 7, 8 ff, 11.

7. Science has had an enormous influence in the world, but it is not the principal cause of the ills of mankind nor a simple cure for them. P. 22; see also the lecture, p. 5.

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Proceedings of the Seminar

MR. WILSON: We are glad to see so many of the scientific people here this morning. These seminars will be held weekly following each of the ten lectures, and are to be confined primarily

to the discussion of science. I am sure that they are going to be very helpful to all of us who have something to do with scientific policies of the Department of Agriculture. These seminars will deal primarily with the physical and biological sciences. We are pleased that some of the scientific people from the National Bureau of Standards, the Carnegie Institute, George Washington University, and other branches of the Government are meeting with us, and we hope that they will be with us during the entire course of these seminars.

Dr. Moulton, in these seminars, we don't try to argue to try to convince the other fellow, but we try to think together and get the issues out where we can see them and compare them and try to understand them. We try to proceed on the basis of group thinking rather than as a debating society. I am sure that we were all very much stimulated by your address of last evening. You have opened up a number of things that the people in this seminar will probably want to discuss during the next ten weeks. Let's have somebody from the physical sciences take the bat. Dr. Deming, you are a physical scientist.

DR. DEMING: A thought comes to me, borrowed from art. I have long been an ardent student of one of the arts (music), yet I have never studied about art; but they tell me that in art some people hold the view that the aim should be not just to portray life as it is but as it ought to be -- in other words, to change it. Just lately I was reading a statement by a modern composer (Shostakovich, I think) who said "There can be no music without ideology. The old composers, whether they knew it or not, were upholding a political theory. Perhaps there is a lesson there for scientists; maybe there is no such thing as science without an interpretation that must change man one way or another. Now Dr. Moulton said yesterday that at the beginning of science, there was no thought of application. It may be, though, that inevitably the thoughts of man in science must lead to applications of science, for either his happiness or his destruction. If that is so, where is science taking us?

No, I can't ask where is science going, because it it going wherever we direct it. The question is, how can it be directed? There is a lesson in a curve. Think of a continuous curve in the xy plane. Now the fact is that over the range in which that curve is defined analytically, any point can be found from certain properties of the curve at another point, i.e., from its position and how it is changing direction. It may be that what science is to be in the future is contained right here at the present time in what we are doing and in the way that we direct it.

Another question is how can men of science make their training count for something, other than in the daily work of research? Just last night Dr. Tuckerman and I were wondering how men of science can be useful factors in the community. Can they do it of their own will, or should they wait until their advice is

sought? I am wondering if Dr. Tuckerman would be good enough to tell us this morning some of the things that he told me last night.

DR. TUCKERMAN (National Bureau of Standards): I want to raise one question that stuck in my mind after hearing Dr. Moulton last night -- his statement that the early aspiration of science was not practical utility.* My feeling has always been that every science has grown out of a human need; but the human brain getting started won't stop with just the practical answer. One of the things that the speaker said was that astronomy grew out of mere curiosity. Rather, it grew out of the attempt to predict human lives by astrological means. You read that in the old Babylonian tablets, where there are calculated horoscopes for finding out how human lives shall go and what your life is worth. Geometry arose in a similar manner.

The basic drive in science, it seems to me, has always been human needs. That is, we want to do something and we try to find out how to do it. Then we get interested in the relationships we find, and go beyond the answers to our needs. Archimedes, I think, is a beautiful illustration, his mechanical problems, and so forth; you find that he pursued his mathematical relations far beyond anything of practical need.

What are scientists going to do to make either their lives better or the world better, or to influence things? I have tried hard to see wherein scientists as scientists are going to do much more in a community than men as men. Maybe they can. I was just thinking about some of the attempts I made to use my scientific knowledge in the days gone by to affect the life of the community in which I lived. That was out in Lincoln (1906-19). For instance, I found that the secondaries of the transformers in the town were not grounded, and I was more or less surprised to find that such a dangerous situation should exist. I thought maybe I had better do something about it. Then I found that as a scientist I might know that the secondaries of the transformers should be grounded, but, as a man, I didn't know how to pursuade the electric light companies do it. I tried my best. I was called a fool: I was called everything on the face of the earth, and finally a man was killed out there. I seized the opportunity of asking before the Rotary Club, how many more people in Lincoln were to be killed. Well, I learned one thing, that though I accomplished my purpose, I did not know how to do it without making enemies. I made some very bitter enemies, and that bitter enmity was reflected later on when some of the men whom I had opposed saw to it that I was tried as a traitor to the United States.

Now, what can a scientist as a scientist do? Does he have any special ability in shaping people's thoughts? As a scientist I can answer a technical problem in my field that is put up to me, but to force my technical knowedge on the world at large in trying to make my

* Pages 2-4.

world better, how can I do it? I thought I would be something of a politician, so I joined the local Democratic Club (laughter). I served as judge of elections, I went around and circulated a referendum petition, and brought home the bacon from Omaha. Thus we got our referendum, but I made more enemies, and in the meantime I was becoming a poor scientist. I learned that I had very little ability to persuade people. Having learned my lesson, I thought I had better try to be a scientist instead of too much of a human being. Was I right or wrong? That is a problem scientists have to face.

DR. DEMING: If a scientist becomes an administrator, must he necessarily lose his technique as a scientist?

DR. MOULTON: My observation is limited. While I was in the University of Chicago thirty years, I avoided all administrative things that could possibly be avoided. I don't think a scientist's ability deteriorates when he becomes an administrator, but his time is often taken up by trivial things -- very trivial things. He answers the telephone thousands of times. He talks with everybody about things of no consequence.

QUESTION: Dr. Moulton, assuming that there has been a change in the incentive of scientific research*, what effect will that have on our basic scientific discoveries, do you think?

DR. MOULTON: You mean now the utilitarian aspects. I think the driving motive will, on the whole, be as it has been. These waves come on in human history. I was a very close friend of Dr. Michelson for many years. I probably knew him better than any other man. I wrote a biographical sketch of Dr. Michelson. The thing that inspired him in his work was the pure amateur spirit. Such men are the men who conceive of the new things generally.

We have younger men coming on that are influenced by the more recent incentives toward the application of science. I said in the beginning that every man speaks from his own point of view; he colors what he says from his own mind. It is necessarily so. I think that the decade from 1920 to 1930 was the decade in this country when Mammon was worshiped, and science was thought to be the thing that would produce the millennium, as a handmaiden for other things. I don't believe we have that same spirit now. Now we are under adversities. We are thinking more of the fundamentals. I don't think adversities are wholly useless in human history or in the history of the world. I think it is true from paleontological records that periods when conditions on the earth have been hardest for the living organisms have been the periods of the greatest changes in the forms of living organisms; and the same thing is true of progress in human history. As bad as the past few years have been in this country, (from the point of view of my early experience they haven't been bad at all) there have been benefits, and

*Page 3.

I think we are getting to a sounder attitude of mind on things than we were ten years ago. We have gone through a lot of foolishness. The decade of the 20's that I spoke of, for example; then more recently the confidence that the Government by some magic can do things. Well, they can do certain things but there isn't anything magic that can run anything as complicated as worlds and societies.

MR. SETTE: I was interested in Dr. Tuckerman's remark * that he had a hard time convincing the utility company that they ought to ground those secondary coils, when the amazing thing was that these power companies had electric engineers who had been trained in science. The second interesting point is that he created enemies in offering his training to the public service. I think scientists themselves are not usually humble. As a matter of fact, we can recall some controversies, for example, when Darwin sprung his famous work he created many a mortal enemy, some of them scientists. I am just wondering whether we should fear making enemies, considering the system as it is, or whether we should train our scientists to be so tolerant that they are never bitter nor hostile to other opinions, and that they will always be dispassionate and impartial.

DR. DEMING: Maybe a person (scientist or otherwise) should take pride in his enemies.

REMARK: Too often science is interpreted as a body of knowledge rather than a method.

DR. SEEGER (George Washington Univ.): It seems to me there are two problems that we have touched upon so far, which although they are related, are nevertheless distinct. One is this: having a certain fund of knowledge, how are we going to interpret that fund to the general public? The answer is certainly a matter of education and may involve many personal elements. The other question which has been touched upon is this: realizing that there are certain basic needs in society today, how can we direct science so as to fulfill these needs? Dr. Deming began by implying that if only we had a point, some place, then by drawing a straight line between the point where we are and that other point toward which we should like to go to, we could get a sense of direction. The only trouble is that we seem to have lost that distant point today. Now Dr. Tuckerman was saying on the one hand, that perhaps originally the sense of direction grew organically out of basic needs. Dr. Moulton was saying on the other hand, that it sprang unexpectedly out of basic curiosity. I am wondering, in a very practical way, if this does not all tie in with a book with which I am sure you are all familiar, written a few years ago (1935), by Julian Huxley, entitled Science and Social Needs. He was lamenting the fact that so much money was being spent on certain sciences and not on others; and he suggested that the direction of science is being determined now largely by the distribution of money to it.

* Page 14.

I should like to offer as a compromise that man is usually starting out on a journey in quest of fulfilling a natural need, but that while he is journeying he has often noted little things, and because he has had leisure and curiosity to investigate them, he has made great discoveries. I don't think any group of scientists planning together in laboratories would ever have discovered X-rays directly, even though there was a crying need for them. So it seems that we need to start on projects that are of definite basic utilitarian benefit, and then have leisure for free play of investigation and curiosity; out of that program, strangely enough, we often find answers to questions we supposed were little related to our problem.

REMARK: Isn't it the tendency for scientists to be utterly objective and purposeless, and doesn't that arise from the fact that they have been dealing with purposeless nature instead of purposeful human nature?

DR. SEEGER: The scientist himself, however, is always purposeful. I should like to mention also this: some people criticize the scientist for being so much concerned about science. Now, my own knowledge of students is that you don't find a good scientist every day, and that when you have a good scientist you had better make use of him by allowing him to develop. Suppose a person comes up to me and says, "Do you think it is wrong to play ping-pong?" I say, "No, but when I have to catch a train I don't have any time to play ping-pong." So, when I am asked, "Is it bad for a scientist to do administrative work?", I say, "No, but when he has a particular problem to solve and there is no other man that can do it, he hasn't time to play ping-pong; he has to catch the train."

REMARK: It is just as difficult to find good minds in other fields as it is in science, and it is not a problem unique to science.

DR. TUCKERMAN: I should like to go back a moment and refer to Mr. Sette's comment*. You must have some other type of training to influence men. The electric light company in Lincoln had very competent engineers, but it costs money to ground secondaries. To persuade them that they should spend that money was a different sort of problem and needed to be handled in a different sort of way from the scientific problems for which I was trained.

DR. TAEUSCH: I am wondering whether there isn't something in science that may be working to inculcate greater humility in scientists, and that is the study of probability that is going on so arduously now, and of the uncertainty that has developed. You intrigued me yesterday by mentioning that when the centre of attention of astronomy was shifted from the earth to the sun, one thing introduced was humility, from the recognition of man's place in the situation. Now wouldn't this recent work on uncertainty and

* Page 16.

probability reintroduce into science a little more humility in the problems it is working with? I am wondering if there isn't something fundamental in this situation.

I think you spoke last night of the fact that the fundamental assumption of scientists is that the world is an orderly sort of thing. I wonder if that isn't presumptuous. Our mental approach to the world is but an instrument that is not very orderly, so the proper scientific approach would be to make some approximations to this world.

DR. MOULTON: I am really learning a great deal about what I said last night. One thing I should like to emphasize is that the most useful thing scientists can possibly do is to examine themselves and have a complete intellectual honesty that we ascribe in careless moments to scientists.

Now, having been intimately acquainted with scientists a long time, I want to assure you that so far as my observation goes, they have all the thoughts that other people do; they are dogmatic, they are -- some of them -- antagonistic, they are like a cactus plant, thorns all over them. Some of these occasionally are obsequious. They have the true religion, and the benighted world hasn't it. Get them together in a group, and they can't agree on anything any more than anybody else. So they should search their own hearts and get fundamentally and thoroughly honest intellectually, and tolerant. I think they are more tolerant than some other people. I think they are more tolerant than philosophers, more tolerant than theologians, on the whole. But they are often not so very tolerant, as I said last night.

Now, I want to speak a word about the question of certainty and uncertainty.* I say again I think it is the fundamental basis of science, that we are dealing with an orderly universe, not a chaos. Otherwise, we should have no confidence that anything we ever observe will be repeated under like circumstances. There is a fundamental difference between indetermination and fundamental orderliness, and our understanding and approximation to it. And clearly, even if the universe is orderly, our understanding of the order of it is subject to uncertainties. We don't know, for example, the size of the earth or the diameter of the earth, within a few hundred feet. We don't know the size of this building within a fraction of an inch. In some fields the uncertainties are not small like those but may amount to fifty percent. Now, I suspect that scientists, especially in the physical sciences, because they are on the whole rather precise in their measurements relative to the ordinary things of life, get a little dogmatic on exactness. There is always plenty of uncertainty. When we get into the domain of biological sciences they are greater, and then into the science of the mind and into the social sciences, they are still greater.

*Not to be confused with Heisenberg's uncertainty principle. Ed.

One speaker mentioned an objective*. I think we here are very closely comparable and our experiences have much in common, but if we should undertake for the next six months to agree on any one objective, we would have very great difficulty, because the uncertainty that was spoken of as to what exists now and what would be desirable, is extremely great. It was not so long ago that physicians were rejoicing that they had reduced the mortality rate of infants. They were patting themselves on the back and thinking that the world would be the same except that the children would not die and people would not have the sorrow of losing them; but a hundred other problems have been raised by that. For example, we have more old people to take care of. So this question of objectives is difficult. That is why I speak somewhat cautiously of improving things.

I said that in England and also in this country there are groups of eager fellows who want to improve things, and I am for them. This or that thing is wrong, and should be changed. Certainly lots of things should be changed. But most of these changes would introduce other things that we would also call wrong or partly wrong. Therefore, I can't see in the immediate future any millennium. I suppose in the days of the Greeks, when they had Plato's republic, it was almost there. It happened again in the times of the Romans, if history is not all fiction.

If you will refer again to what I said about the orderliness of groups,**you will see that there is orderliness in it from a certain point of view that we couldn't prove, yet one could never prove the opposite. There are currents in it that look to us to be conflicting. However, those are the things that give it an over-all order. There is not any over-all order to an absolute chaos. That is, to my judgment, logically impossible. The understanding of this orderliness that there is in aggregates is an important thing, but very difficult to comprehend; if that were understood, our social outlook would be different. It would be less rigid. We have got to expect these sub-groups or the elements of our large aggregate to have conflicting motions, if we speak in terms of molecules that go in different directions. If they went in the same direction then the over-all orderliness would fail. The averaging of all of it is the important thing that gives something substantial. I believe in there lies the philosophic basis, the fundamental basis of democracy as compared to an autocracy, but of course it is not easy to elucidate.

REMARK: It has no meaning to me that on the average people are going to die at seventy if I am going to die at fifty-five.

DR. MOULTON: It does not happen to you particularly, but that is an important fact just the same. To all life insurance companies it is important. It is important for production. That is more important for the group than any specific thing you could possibly say.

* Page 13. ** Pages 8, 9, and 10.

MR. WILSON: I was at a state university recently and I visited with a very distinguished chemist there a little while. We talked about specialization. He said that he had an old textbook that his father had studied, I guess about the time of the Civil War or thereabouts. The textbook wasn't called science, it was called Natural Philosophy. He had occasion recently to look over that book and he read the first two or three chapters and looked at the table of contents. It was so attractive to him that he took a couple of evenings off and read the whole book. He said that his father was a man that wasn't a scientist but as he looked back on his relations with his father and the influence of his father on him, that he could see that his father had a conception of the philosophy of science, a kind of over-all view of what science was, and so on, and that he got it out of that book that was called Natural Philosophy. This man said that he raised a good deal of question because of the narrowness of training and the high degree of specialization in our high school and college education. In the broad fields of science many educated people--college graduates--do not show depth. He remarked that the great weakness that we have at the present time grows out of that narrowness; and that in some way, or somehow, with the great expanding field of knowledge here of nature in the world, that there ought to be a kind of selectivity, or relativity, or something of that kind in the teaching of science. You have had a great deal of experience and have thought a great deal along that line, Dr. Moulton, and we would like to get a little of your thinking on it.

DR. MOULTON: He was making a plea for the point of view of the amateur naturalist. In those days they had two sciences, natural philosophy and natural history. And there is something extremely fine in the attitudes of men who had that point of view. There are still plenty of men. I was a close friend of Dr. T. C. Chamberlain, who was president of the University of Wisconsin before he went to Chicago. His mind roamed all over these things, although he did some fundamental and very fine work on classification and those things, and had an outlook, a balance, a breadth, kindness, toleration, and penetration that was amazing and beautiful. I agree that science probably on the whole is taught too much from the point of view of the specialist.

In the University of Chicago we introduced a general survey course that was, on the whole, very successful. I think it was required of all freshmen, at least of all who took a certain major sequence. It went over the physical sciences, i.e., the physical and biological sciences, not the social sciences. Later, after my day, they introduced social sciences. Now I think that although it wasn't deep in any special line, it was an extremely good thing. I ardently advocated, somewhat facetiously, it is true, in an article I wrote for the University magazine, that every member

of the staff of the University of Chicago be compelled to take the course, because--it was part seriously and part in jest--the course was assumed to be so important that a person couldn't be considered educated without having been exposed to it. Now naturally the faculty shouldn't be below the standard of graduation for the college. And then for another reason I advocated it--they actually needed it. The faculty were narrow specialists, most of them. I took it myself and shocked others. I advocated it so that they would learn from actual observation how poor and narrow a lot of instruction is. Several of them who took the course said afterward that they thought it was a very good experience.

In the preparation of a book that was later published, I was educated enormously in various ways. Well, when we wrote this book, we expected to produce it in a short time--a few weeks. Now, having written a considerable number of books, I didn't presume that a good one could be done in that time, so I recommended and insisted that I would not cooperate unless each man in the group would read his chapter to all the others for their criticisms and comments, and they said, "Well, that is all right; it will take a few days more." I said, "Sure." Mine was the first chapter in the book. I said I would have it done in five days, which I did, the first draft at least. It took us five months to read that. Here were men on the whole who occupied important positions in the University, and the differences in outlook in all these qualities which we think of here as desirable and undesirable, cropped out, and the differences in points of view were enormous. Meeting together once a week for a whole evening for five months was an education. So, from that again, I observe that there is a difference in conflicting currents among specialists in science in the same institution, the same age, apparently the same background. Over-all we had an average. When the book was written, on the whole it was a pretty good book.*

MR. BEAN: Dr. Moulton, yesterday at a point in your paper where you referred to a recent trend in scientific thought in England you said that there were some scientists in England who in the past decade or two had gone off the track (not your words, of course) into the field of metaphysics, mysticism, things of that sort, and you were magnanimous enough to suggest that that was the result of the trials and tribulations that the generation of scientists in England had experienced through the war and its complications.

DR. MOULTON: I think it is true in some cases.

MR. BEAN: And today you threw out the suggestion that scientists tend to turn to more fundamental,** more scientific

* The book is <u>The Nature of the World and Of Man</u> (Chicago, 1926)
** Page 15.

thinking in periods of stress. What explanation would you have when you find the same kind of tendencies among American scientists, who, after they have pursued their specialty to the apparent end point and find nothing more perhaps than energy or something else as the basis for material things, find themselves slipping into metaphysics and other questionable branches of science? What explanation do you have for the American scientist falling by the wayside?

DR. MOULTON: Well, in this country we didn't have the personal losses that they had in England. They suffered tragedies that we in this country can't realize. You who have been there and know some Englishmen will get the point of view. That accounts for some of these. That is a suggestion. We are not any more perfect mentally than we are physically. We go off on little tangents. As the 20's passed along, the silk shirt era passed, and we saw the wreckage of war in Europe. At first, you see, we are under the illusion of the Armistice and the peace, the impossible terms that were imposed on Germany; failing, failing, failing, all these things failing, the League of Nations failing. The books that came out, some of the plays, motion picture plays, showing the tragedy and failure of it, the cost in this country for the veterans went into billions, nine billions, I think, approximately. The disillusionment period was coming on. All that period was one of gradual disillusionment. And then the depression came on, so we aren't psychologically so very different from those, except personal losses, very different from the rest of them. When we get into trouble we commence to think about it. That is human nature. We feel our helplessness. Then we get into speculation as to what we regard as more fundamental things. I don't say we are right, that they are more fundamental. But we are in the position where we are reflecting more than we did in the 20's on the destiny of man. We are a little more doubtful about the millennium being just around the corner. Everything that has been tried has failed. At least it hasn't approached the promises or the expectations. We are a little confused and depressed at the moment--most people. I am not in the least, because this is temporary.

We are groping now. The problem is how to do it. I repeat what I said; I don't see any easy solution; but hopefulness and a belief that there is an orderliness in the universe, a knowledge of which will help us, even though it is difficult to discover it, I think is a steadying thought.

MR. BEAN: Would you say the disappearance of civilization is one of the orderlinesses?

DR. MOULTON: Many civilizations have disappeared. The dinosaurs had the world for about seventy-five million years, and toward the finish they might have got together, as we are, and said, "Boys, we are sitting pretty."

DR. TAEUSCH: May I ask this question: I was much impressed with your statement of fundamental orderliness, and it brought out a certain point. We have been dealing primarily with factors and such in our measurements. Do you carry that over to the fundamental orderliness of thinking, in the mass, at least, and possibly to a lack of understanding in our own individual and group thinking, which may for the time being appear chaotic?

DR. MOULTON: Yes. Let me just comment on that individually about our thinking. It refers to Dr. Tuckerman's comments on my thesis that the inspiration for the discovery of such disparities was not to make the motor but to find out how things work. In my opinion, which I state with caution, our minds are the products of the things in the world for a billion years. We have been in the process of developing, roughly speaking. Our ancestry back over all that time has been subject to the rhythms that are in nature, the succession of day and night, of the seasons, of all the other things, of all these fundamental properties of the universe. They have been impressed on us physically and mentally, and whatever we have now has come up through this marvelous history, and in that sense, at least, our curiosity is something that has been acquired in the experiences of our ancestry. Certain low forms have remained constant hundreds of millions of years. So we are up here to this stage. So in our minds there are impressed on us certain fundamentals, but expressed in very great variety, and there is a mass orderliness and I think there is a degree of coherence in the individual. But the mass of us, the judgment of the mass is better than the judgment of any one person or any small group of persons. Now I may be wrong in that.

MR. WILSON: I see that a whole lot of people are ready to spring up here to ask for a definition of science, so I think we had better adjourn.

(Meeting adjourned at 10:30)

Ten Lectures and Discussions on

SCIENCE: Its History, Philosophy,

and Relation to Democracy

held at the Department of
Agriculture Graduate School
under the chairmanship of

M. L. Wilson
Under Secretary of Agriculture

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Edited by W. Edwards Deming
Assistant Director of the Graduate School

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Lecture II

THE DEVELOPMENT OF CULTURAL AND SOCIAL VALUES THROUGH THE RELATION OF SCIENCE TO OTHER MAJOR FIELDS OF ACTIVITY

by

John C. Merriam

The Graduate School
The Department of Agriculture
Washington
1939

DEVELOPMENT OF SOCIAL AND CULTURAL VALUES THROUGH RELATION OF SCIENCE TO OTHER MAJOR FIELDS OF ACTIVITY

By John C. Merriam
President Emeritus of the Carnegie Institution of Washington

In the auditorium at 5 o'clock Wednesday
15th March 1939

I

The subject on which I have been asked to speak today concerns science with particular reference to its influence in developing social and cultural values of broad application in the advance of civilization. The emphasis is not placed specifically upon the contribution of science alone, but is laid rather upon results obtained by linking of scientific activities with those in other great fields of human interest. Although specific values are developed by science functioning independently, a much greater influence is exerted where its activities are related to those in other fields.

The point of view from which the subject is now examined represents in considerable measure the present-day development of science through large use of synthesis in addition to the analytical methods so characteristic of research in the period only a few decades behind us. Advances of analytical science in the past naturally carried us into more and more extreme stages of specialization, until some of the subjects investigated seemed widely separated, and science as a whole appeared to express less of unity than was once recognized.

More recently, the synthetic method of study has in some measure replaced the analytical, and rapid progress has been made by bringing together fields of research once appearing rather widely separate. Through this method new generalizations and principles have emerged, and by use of these new ideas further specialization has been possible. As a consequence, the whole of science, with its better organization and better perspective, has advanced more surely as well as more rapidly.

One of the ideas that I have in mind with special reference to the relation of science to cultural and social advances is that the values of science should not be separated too sharply from those of other subjects, but should show interlocking with related fields of thought and action in such manner as to give better vision and better understanding.

Among the subjects that one might naturally consider in their relation to science are philosophy, art, religion, education, economics, and government. Definitions of these fields that we might give would naturally be modified according to the purpose or point of view of those concerned in discussion. But science itself would be accepted generally as concerned with fact-finding, organization, and interpretation of knowledge, with emphasis on rigorous adherence to truth. Here, there may be no deviation from

truth, no compromising. Invariably the question asked is: What precisely is the situation or the mode of operation involved?

Science builds in all directions, forming great complexes--structures such as might be represented, for example, by the Tower of Babel, built high and wide--where, you will remember, the work stopped, according to the record, because the workmen could no longer talk the language of their fellows. If this had been a great scientific project, I would be inclined to think that they specialized so far in various directions that their terminologies and techniques became unintelligible to any but experts in special subjects; just as, in these days, techniques and terminologies of science sometimes are understood only with difficulty by our friends.

The story of the Tower of Babel, as we know it now, is commonly visioned as referring mainly to the fact that they build high toward the sky. The account refers to people going out on the plain of Shinar where they made bricks and had slime for mortar, and they said: Let us build and "make us a name." The Lord thought that was not good, just to make a name, and he said "And now nothing will be restrained from them which they have imagined to do." If they accomplish this they will be able to do anything else, so they should perhaps be held back. At any rate, their tongues became confused; they did not understand the language of their fellows. There is a good lesson in this for all kinds of science and of business today. If we cannot understand one another's language we build no great structures, whether they be towers, great scientific organizations, armies, governments, or other things.

II

As this lecture course is devoted in part to the study of democracy, it may be well to say that, while I am a great believer in democracy, I think of it as being one of the tenderest plants that human kind has ever nurtured; that it must be handled with great care or it cannot develop. It requires for its proper growth balanced judgment, give and take, and mutual understanding; otherwise it cannot be made to operate. Is it conceivable that it might make a record in history like that of the Tower of Babel?

It is only fair to state that one difficulty in attempting to keep full understanding among men in the study of great questions arises from the fact that the problems are often so wide-reaching and complicated that vision over the whole subject is difficult. This I found illustrated well many years ago, when at the Carnegie Institution of Washington, I brought together a group of scientific men concerned with genetics or inheritance along with others investigating ecology or environment, to see if we could not obtain a better understanding of the difficulties found in study of the relation of heredity to environment. We did not succeed fully. As we came out of the meeting one man surprised me by saying "I think the poet Bryant in creating his Thanatopsis probably saw the situation in nature from the scientist's point of view as well as from the poet's, when he wrote: 'To him who in the love of nature holds communion with her visible forms she speaks a various language.'" He thought that the influence of the "various language" of nature had been illustrated in the meeting just ended.

Science as a special field we can understand as to its general content and purpose. But difficulty arises when we attempt to relate it to the other subjects with which I believe it should be connected if we are to make the progress that the world ought to show through the ages. Careful study is desirable if we are to bring it into the most effective touch with philosophy, government, art, education, religion, and other subjects of comparable importance. The contribution that these relations can make to the increase of our broad range of knowledge, seen as culture, and our appreciation of those values that are fundamental in social evolution, makes it imperative that we attain clear understanding of these contacts among the various fields of activity.

III

In developing this question it is important first of all to indicate some of the types of the relation of science to subjects of similar importance which have been mentioned. This is done with a view to considering the desirability of finding the best means for cooperation of science through relation to these fields of thought in order to make great advances in culture and social understanding possible. In a later portion of this discussion there are presented a few illustrations of the type of interlock to which consideration may be given.

Philosophy. Early in my career as administrator, I received a distinct shock upon hearing a distinguished gentleman, president of a great institution, suggest that I was too much inclined to allow persons working with me in science to take a philosophical point of view. Without stopping to think, my reply was that I did not believe there could be a science without a philosophy, that philosophy was an attempt to fit together the various elements in their proper adjustment to one another and to interpret them in terms of logical relation; that these things had contact each with the others and we must discover what that relationship is. It may be important or it may not be, but we should at least know what the connection is and give it the proper evaluation.

Of course there are things that proceed under the name of philosophy that can be described as merely making the wheels go around without accomplishing much. Such situations I understand, but there is a great deal of philosophy that is based upon sound, solid fact. A considerable part of it that comes out of science is philosophy that will carry us a long way. On the other hand parts of science that refuse to recognize the importance of philosophy, will, I am afraid, just go about in a treadmill without reaching far. So I am strongly in support of the idea of having philosophers and scientists come to understand each other sufficiently well so that each may use the methods of the other.

Art. Many ask, what can science and art have in common? Art may be considered as a purely emotional relation concerning beauty and elements of that nature. What does that have to do with the matter-of-fact development of a scientific program? Of course you can accomplish much if allowed to make your own definitions. When I discuss subjects of this nature my definition of art may differ from that used by some. But artists often accept it. Art to me is a way of presenting things in the most effective possible manner to show their real nature and significance, the best possible picture, with the spotlight turned upon the thing shown so as to give us the clearest view including the best relation to its environment.

Many scientists have used art of just that kind, and I have known some who attained eminence in science, by using that method. One of those was Dr. A. A. Michelson, a distinguished professor of physics in the University of Chicago. I talked the question over with him often. He said "There are not many in science who accept it, but I believe that the more artistic the statement of a problem in science, the larger the chance of presenting the truth." So I find that widely through science the artistic method may not be expected to do harm. And it may do much good. I cannot say much concerning what artists believe with reference to the use of science in art, but I feel that more and more they accept the scientific method of approach as one means of gathering the data needed for better presentation of certain kinds of subjects. So I believe that as time passes a cooperative relationship of importance may be developed between these fields, and that the contribution to culture and to social values will be large.

Religion. Although religion presents some of the most difficult problems, seen from the point of view of one considering cooperation with science, it is desirable to examine the possibilities of profit in study of this relation.

The place of religion among the varied interests of life, ranging from science to esthetics in one direction, or in another direction, to business, differs according to specific interests of individuals or according to the nature of the background. In general terms, religion is commonly looked upon either as the relation of man to his Creator or as an expression of the relation and responsibility of man to other men. Relation of man to his Creator is perhaps most commonly interpreted in the sense of personal relationship to a supreme being. From this point of view, creation of the individual would be interpreted as a personal act. But if personality of the Creator is eliminated from consideration for the moment, it is still possible to discuss a relation of man to the surrounding universe out of which he has developed. Modern views of science tend to describe the world as something not well within the limits of precise delineation or description. It is seen as something greater, grander, and much more wonderful than anything we have been able to describe; more wonderful, some would suggest, than the particular personal God which individual mortals have visualized.

In the sense of relation of man to a creator represented by the universe about us, questions of right and wrong, or of obedience to laws of the Creator translate themselves into consideration of obedience of the individual with reference to those laws of the universe which control human life. It might be as serious a matter to transgress the laws of nature representing the Creator as it would be to transgress laws of a creator expressed in terms of personality.

From another point of view religion is considered to represent factors that concern relation of man to his fellow men, and in this respect the matter of touch with the Creator seems to be modified, since relation to fellow man is distinctly a different situation from that involved in our touch with environment in the sense of the universe. The problem of the relation of man to man involving opportunities, responsibilities, liberties, and obligations as we see it in the study of forms of government, and especially in a democracy,

constitutes one of the questions in which science, government, and religion interlock.

From both points of view, that is, considering the Creator as in or behind the universe about us, or considering the Creator as in the world of people about us, we have a vast amount to learn from scientific study and investigation as to what our situation really is, and as to what our obligations and opportunities actually are. It may be that science has a great responsibility for telling us something about what is involved in these several situations, at any rate concerning certain aspects of religion. And until that has been outlined, so that we see it clearly, science will not have done its duty.

IV

Referring to the relation of science to other fields broadly, it is my belief that one of the things most needed at the moment is that kind of synthesis of the subjects discussed which will give us a framework of organized knowledge, rather than a huge mass of loosely connected facts, an arrangement in which the points of view represented by these various subjects will be so worked out that we shall obtain an understanding of the position of man in the world and of his relation to others. We should have also an appreciation of our opportunities and of our responsibilities, a knowledge of the materials with which we can work, and a vision of what we may accomplish. The advances suggested can not be carried out by science alone. Cooperation of investigators in these various fields is what is needed.

One of the difficulties that we find in attempting work on any question of this type is the idea that this is what we call a practical man's world, and that the kinds of things that I have been mentioning often seem relatively unimportant. This is also sometimes called a machine world. From another angle of vision it is a material world. In another sense it is a world in which people have seemed more important than what is seen as activity of the Creator in the world about us. But personally, I see the world as less clearly material than it is often pictured. Those who say that they have no philosophy and no religion commonly have makeshifts which they bring forward on special occasions. These substitutes are usually poorly constructed, and perhaps largely negative, and they do not function well in the place of systems which have stood the test of time. Even for the maintenance of living conditions from day to day it is discovered that the kind of philosophy made up on the spur of the moment is not often satisfactory. We do better with a scheme in which instead of a purely material point of view we attempt to live, to some extent at least, in the realm of the philosophical or the spiritual or the religious which seem to be natural elements of the atmosphere in which human beings thrive.

We admit generally that the time is past when one can say that maintenance of life depends solely upon daily bread. Christ said that man may not live by bread alone. This may be stated in many ways. It is necessary that the educational program of today furnish substitutes for the imperfect systems of philosophy and religion and life upon which people are leaning in the place of the higher life. The schools, from universities down through the colleges,

high schools, and all the lower grades, have a great responsibility for bringing into the lives of persons at all stages that which represents real value, which may give to everyone the things that can be maintained through life and bring joy continuously, and which may be substituted for what we call maintenance of life on the basis of purely materialistic conceptions.

V

The significance of interlock between science and other phases of human thought and activity that have been mentioned, and the contribution to cultural and social values, is best shown by illustration of the manner in which these relationships develop under conditions presented by special types of interests in life. For this purpose it appears desirable that one select the values involved, on the one hand, in considering the meaning of the surrounding universe or of the realities in nature as they affect human thought and action in normal living; and, on another side, to examine some of the factors in our relation to the world of people about us. A further phase for examination of this contact of science is chosen from study on the problem of what are commonly called cultural and spiritual values and their human significance, as involving the interlocking of science with other attitudes of mind.

Influence of Nature on Thought and Life

I have referred already to the fact that imprint of the realities in nature upon human thought may have great significance. The longer this problem is studied, the more surely am I convinced that a tremendous loss is suffered by the great bulk of all humankind through failure to recognize the contribution of nature in giving from day to day expressions of grandeur, and beauty, and soothing qualities forming a continuously growing scheme of values that accumulate over the years. Collectively, these features give something of the utmost importance, features that may be as full of meaning for us as anything obtained in life.

There are, I know, those who feel that the beauties and the joys of nature have less and less importance as the years go by, and that the mechanized world, as represented by automobiles, movies, and comparable things, has taken their place. I regret to say that in a large measure they really have taken the place of much that is in nature. Somewhere in the west, recently I learned of a gentleman who inquired where he might go to drive. When asked what he wished to see, the reply was "I am not interested in scenery; all I want is to drive fast." He wished a good road in a national park on which to enjoy himself by driving at the maximum speed permitted. Grandeur and beauty did not interest him. Of course he could have found a good road in a less spectacular region more easily.

Everywhere in the world the joys of nature can provide much for us. These values have been interpreted in some measure by a few great students. Unfortunately only a relatively small number of persons seem really to have discovered in nature the elements of largest significance, and to have defined them so that they can be located and appreciated. Wordsworth stands out in this group, but significant are also the works of Tennyson, William Cullen Bryant, Shelley, Keats, and others. The features that I have in mind hold for

us large possibilities of joy and comfort, and we are discovering now that scientists can give important aid in the interpretation of nature: that instead of breaking down the values of appreciation, science helps to develop them, so that as study proceeds we find more and more elements giving us these greater pleasures.

Some years ago one aspect of the meaning in nature was impressed upon me especially by a poem found in an old magazine. It concerned three phases of the year, and was based upon the idea of destiny, individual, and for mankind in general. There seemed in one portion of it an exceptional connection with the kind of help that science should give in statement of such relations. The first of several stanzas referred to spring and the stimulus it gives. It was as I recall it:

> "Who walking in the spring may see
> Fresh green upon the poplar tree,
> And smiles with hope as he goes by,
> Begins to see his destiny."

Perhaps more than through any other aspect of nature, the opening of spring makes one consider opportunities for development, the expectation of progress or growth within ourselves, and the possibilities of our own destiny.

The third part related to influence of autumn woods. Perhaps you have felt the depressing effect of falling leaves and approach of winter expressed by naked trees. Bryant has written,

> "The melancholy days are come, the saddest of the year,
> Of wailing winds and naked woods, and meadows brown and sere"

The poem which I found turned thought regarding fall and winter in another direction. It read:

> "He who can see the glory fade
> From noble works that God has made
> And keep faith fresh in his soul's eye
> Is master of his destiny."

It happens that as archaeologist, and palaeontologist, and geologist, I look out to view the story of mankind extending back a million years or so, and see beyond that the earlier record of life reaching farther down for many more millions of years. Research has shown us that in all of this long stretch of time a continually advancing sequence of stages in evolution or development coming up through the ages. In reading this story, always I have the feeling that, however dark the skies at a particular time, apparently the lesson of history indicates that the trend of things as recorded in nature is toward the better.

With special reference to the little poem to which I have referred, it took me long to reach a stage at which I could watch the melancholy days come and the leaves fall without being depressed, but I am now able to see the glory appear to fade from these noble works that God has made without being downcast

or discouraged. Some years ago I expressed my feeling in an article pointing out that, without noting the absence of leaves, one can look through the branches of fall and winter trees to see their beautiful structure, and also note the glory of the sky beyond which is not easily seen when leaves are present. Also we have faith that the fading glory is only one stage, and that life and beauty and joy will come again with spring. And the lessons of science help definitely in shaping one's faith regarding the orderliness and dependability of nature, and concerning the dependability of the universe in which we are placed over the longer periods of change where hope seems almost to fail us.

As science advances its study of the world about us, more and more it brings out new points of view, new vistas, and visions of great regions which show nature as much more wonderful and more beautiful than we have found it without the eye and the mind of science. These studies in most cases reach a stage at which the beauties discovered may readily become the objective of constructive work by the artist. They lend themselves also to thought concerning illustration of power behind the universe, or by students of religion. So, in this region of nature study, we find value in bringing the work of science into relation to that of art and religion with advantage to all, and with opportunity for greatly enhanced appreciation of what nature has to give us.

Human Relations

Just as in scientific study of nature great elements are brought to light which enhance our appreciation and contribute new values, so in examination of relations involving our fellow men new and significant things are discovered which have their principal use in fields of activity or of thought commonly separated rather sharply from science.

Business. But even in the more or less prosaic human work of making a living, or what we call maintenance of life, it appears increasingly desirable to recognize the importance of relating scientific methods to our modes of procedure. This is true even in what we call the field of business, just as it is sometimes necessary to secure cooperation through interlock with certain forms of art if the best results are to be obtained.

The trend of human affairs has made it abundantly clear that in all fields of activity every individual should consider it a responsibility at least to make acquaintance with what are called the methods of research, and to recognize the necessity for insisting that everything with which he deals be settled on the basis of facts well organized and clearly stated. It does not matter what the position of the persons concerned is; all classes down to the humblest citizens should build life on a framework of truth, well tested, well organized, and honestly used. The research spirit finds its application in every kind of business every day. No one engaged in any activity fails to come frequently upon situations in which there appear new things to be discovered. Whether making breakfast food, or in the steel business, or operating a farm, one is continuously encountering problems that mean investigation. You may hire someone to do your research for you, but the first step is to outline the problem, and the scientific spirit of the man who initiates the work may be as important as that of the trained investigator.

Government. In human relations one of the most important aspects of action concerns that great field of thought included in government, or formulation of methods by means of which human beings may live together with such organization as will give the maximum of opportunity for each individual, with the greatest strength and efficiency for the groups as a whole in the interests both of individuals and of the entire body. Necessities of the case have developed some forms of government by the trial and error method and others by careful research and planning. Some have assumed that where the ideas and desires of individuals are concerned, scientific research would not be applicable. But even accepting large freedom for the individual it is still true that government and all that it comprises must develop on a foundation of facts and of principles derived from study of these truths in ways comparable to the methods of scientific research.

So research in government, and economics, and finance, and all of the elements comprised in these important but complex human relations has grown to greater and greater importance. And in this development the attitudes and modes of operation of science have come to interlock with those of other phases of human interest and activity. Today we appreciate the necessity of bringing to bear in this constructive work all that science and philosophy and art can furnish, and of so relating these subjects to each other, as to produce the forms of human organization best suited to give means for maintenance of life, opportunity to grow, and the joys of appreciating life while it is being lived. Which things I understand to be meant by the "life, liberty, and the pursuit of happiness" so well defined in the Declaration of Independence.

Now that the world has passed through many experiments, we are finding that even this democracy, toward which our belief turns so hopefully, is something that must be examined with all the care that research can furnish, in order to avoid the mistakes that seem possible, even where the will of the people is assumed to be supreme. And it would appear that unless our government is guided in some part by well founded science as well as by philosophy and religion, we may fail to attain the ends desired.

Cultural values. We know that as commonly visualized, human life comprises largely those practical things that contribute toward the making of a living, or the business of the world concerning what is seemingly of immediate importance. But the content of life so defined, while of greater complexity than that of the beasts, will show itself to differ essentially from what appears in the groups or strata of creatures considered as below the level of man, through the features that we describe as intellectual, cultural, and spiritual. The kind of activity which we have recognized as science, with its acquisition, organization and interpretation of knowledge, is classed in the field of the intellectual. The broad pictures of cultural and spiritual values have significance somewhat different from that of the strictly scientific field of intellectual interests.

Without attempting to prove that influence of the scientific attitude extends farther than is assumed to reach, there may be advantage in examining the possibility that here also cooperation of science with activities of other types may lead to significant contribution.

In this phase of study, concerning what is furnished by science, consideration is given to relation of science to the two phases of human interest included in the regions of the cultural and spiritual, as commonly recognized.

If I were to contrast science and culture, I would say that science is an attitude of mind which has to do with the securing of facts, their organization, and their interpretation. From my point of view culture is the highest refinement of our appreciation of knowledge in the comprehensive sense. It is that wider vision which gives extended view of all that touches the human mind, putting each phase of knowledge in its proper relation to the others as seen through the balanced mind of what we call the fully rounded man. I am especially concerned with the idea of developing the right relation between the peculiar vision and factual foundation of science and that broad picture of knowledge represented by culture. The "culture" to which reference is made in the title of this paper illustrates in considerable measure this point of view. But, even though certain cultural values are seen as one result in contact of science with other fields, it seems important to examine the relation of science to culture itself.

From the nature of the case, culture will be looked upon generally as conservative, since it is that view that shows us knowledge of all aspects each in its proper relation to the others, and it gives us a situation in which we have balanced judgment concerning the significance of these aspects of knowledge. There is danger, however, that culture may become too conservative. There is no finished chapter and no closed book. Knowledge advances continually, and culture must be expected to receive new things from time to time to add to its general scheme. After all, culture must be constructive and not passive. It is the man with balanced judgment who sees the world as it is, human beings and their surroundings, with everything in its place. It is that man whose judgment is most important; but it must all be used for constructive purposes and not allowed to become destructive or merely passive. So I see culture as a constructive element representing the wider range of interests, sometimes involving science and all or much that it furnishes us.

As a student, many years ago, I was impressed by the fact that science clearly represents truth, and that culture as commonly visualized leads to art and to beauty, and that truth and beauty are among the most fundamental influences in life. There are many who are not able to see that truth and beauty have relation. The philosophers will indicate to you that this question began with the Greeks or earlier, and has been covered by discussion and song down through the ages.

Some months ago I picked up an old copy of a University of California magazine, edited by a mathematician, a publication that up to the time of its death, quite recently, was devoted largely to poetry. I was interested to find there an article by a student concerned with research who took time to write the following lines regarding beauty and truth:

"Beauty and truth share one abode, I know--
It is fallacy that truth is plain.
As beautiful as mountains tipped with snow,
So loveliness adorns the truth's domain."

It was not new; the Greeks said it, and many others all the way through; but it is exceedingly important for us today to realize that there may exist and should exist this relation between these two often sharply separated aspects of our interest. The future of science and philosophy will make this relation clearer and more important to us--perhaps we can help in the work.

Spiritual values. Seen from some points of view, what we call spiritual elements may appear to represent the farthest stages of removal from so-called practical living. But to many the value of spiritual things overshadows all other features. The contrast is especially clear when one appreciates the fact that spiritual values seem the opposite to the immediately practical because they are the formulation of what we sometimes describe as the "eternal verities". They concern ultimate values, the basic significance of things and the real essence or actuality of their being. The fact that the spiritual may not always be described with exact meets and bounds or measurement does not mean less reality. It may mean that those who pin their faith to so-called practical realities represented by exact measurement may be farthest from true understanding or appreciation of the real factors involved in existence and in life.

A broad view of the universe of things and of life impresses us with its vastness. It shows no beginning and suggests no end--and yet we are wholly clear as to its reality, its moving, growing, evolving being. As science advances we suspect dangers in a complete separation of the so-called material and non-material. Physics carries us back to what are almost intangibles. The clearer we are regarding the broader and deeper reaches of things physical, the nearer we may seem to come to what is often set off in the realm of the spiritual. Science has difficulty in reaching the limits glimpsed in the physical, and we may not therefore be disturbed because we are not able to apply it fully to what we call the spiritual. But through everything we see running the binding and controlling threads of essential truth.

The clear unity of nature, with all its variety and the expression of its many forms of procedure in activity that we call laws, cautions us regarding the assumption that we can justify full separation of these things from other phenomena that we call spiritual, but that also exhibit evidences of underlying unity in which definite modes of procedure seem to express themselves.

Whatever else be true, we seem to find what we call the eternal verities as the most impressively real values, and as showing most clearly the character of our environment--or our own nature as a part of this scheme out of which we seem to have come. We realize that however superficial our formulation of these things, we must keep science in touch with the materials of the so-called spiritual field.

Wherever we look there appear, on one hand, the relations between the factual materials shaping themselves to indicate modes of procedure called laws in science--and, in fields close by, we see those other varied types of activity known under different names and with what appear to be other functions. But everywhere there is indication that the proper relation between these other types and the activities of science gives new or increased values, and advances our position in the world of things social and cultural.

SEMINAR

Following the lecture by Dr. Merriam
Office of the Under Secretary
Thursday morning at 9 o'clock, 16th March 1939

Syllabus

1. An account of an attempt to give college students a general course in science; pp. 37-38.

2. The lecture method vs. actual experimentation; the former is needed in order to get the benefit of mature minds, but the latter is indispensible. Pp. 38-39.

3. Can the same method of approach be used for social problems as for physical problems? Pp. 39-40.

4. Some remarks on the scientific attitude; pp. 40-41.

5. The truth will eventually come out, and should not be forced; p. 41.

6. Some remarks on the synthesis of different views and observations; get them all out and look at them. P. 42.

7. Should philosophy be developed by specialists in different branches of science, or by persons who attempt to glimpse the whole of science without becoming experts in any one science? Both are necessary. P. 43.

8. An account by Dr. Atwood on a new plan of teaching at the University of Florida; pp. 44-47.

Proceedings of the Seminar

DR. BLAISDELL: Dr. Merriam, we all regret exceedingly that the Under Secretary is ill and confined to his home and can't occupy the chair that you are occupying. (I would find another chair for you in that case.)

This series of discussions is part of the series of lectures, and the discussion moves from one point of view to another without any participant trying to impose his point of view on anybody else or on the group as a whole.

I am sure that everybody here who heard Dr. Merriam's lecture last night was impressed with the wide scope of the field that was covered; that ought to give ample opportunity for questions and opportunity for different points of view to come to the surface.

DR. WINTERS: Mr. Chairman, last evening Dr. Merriam spoke of the need for a new generalization, the generalization that would take into account the integration of branches of science with each other and with our social problems. In view of an intense specialization that we now have, I raise the question as to how our educational system can bring about the integration that is needed to give a student this general view. Shall we attempt to develop generalists or generalized groups, or shall we attempt to use a more generalized education to all groups?

DR. MERRIAM: That is a proper question and I hope I have some answer for it. It is the kind of a question that I attempted to consider while a professor in the University of California many years ago. Some of us there came to the conclusion that scientific education ran too far into specialization, and that science became separated too far from the other fields of interest. We discussed the matter with the President, with the result that a committee was set up for the purpose of giving what you now know as a course in general science. That course consisted, I believe, of twenty-eight lectures; and I think there were fourteen professors participating. The course seemed to be a success. At any rate, after these many years, I am still meeting quite frequently students who took that work, and they say that in spite of the fact that they received only a quarter of a unit credit, it was one of the interesting things in the college course.

The lectures covered the whole field of science. They were very simply stated, but were effective. They were given by very distinguished people. They represented fundamental things in science, given with a clear form of statement. They made a deep impression, and brought about that integration of the sciences of which I have been speaking. Also they brought about, to some extent, the integration of science with other subjects. I believe in that system, only I would integrate farther and in some instances bring science into relation to art.

I think it is very unwise to allow students to go through a university or college course without obtaining some idea of the meaning of the major fields of

science or knowledge generally; not that they should have a smattering of everything and no understanding of anything, but that it is desirable to have some understanding of the general field.

I remember that the man who gave the lectures in geology in the general science course, a very distinguished man, who is at California still, and he said, "Gentlemen, I am to give two lectures: my first is concerned with the forces within which are constantly moving the crust of the earth, sometimes up and sometimes down. My second lecture will be devoted to the forces in the atmosphere which are continually bringing about the erosion, or the destruction, or the modification of the surface of the earth." So he proceeded with those two things, one played against the other. At the finish he had illustrated with a fresh point of view what is in the immediate environment of Berkeley, something everybody there should know.

DR. SEEGER: Dr. Merriam, do you believe that the students will get an appreciation of the methods of science by attending lectures on the various fields of science, or should they have a more intensive study of some one branch of science in order to appreciate its methods?

DR. MERRIAM: The lecture method is evidently necessary in order to bring carefully digested results of the thought and experience of investigators together in such form that the mind can form a picture. But it is also a tremendous handicap because we very soon put the lecture in place of everything else, and everything is done merely by talking about it. Have you been a professor?

DR. DEMING: Dr. Seeger is Professor of Physics at the George Washington University.

DR. MERRIAM: I have been a professor also and know the advantages and disadvantages of teaching. Some of the disadvantages are that you do so much that after a time it becomes second nature and you may almost talk too easily. The thought back of what is said may shape itself without meaning much, and after a time the lectures may formulate themselves without meaning much; and the textbooks do that. It is a dangerous thing, and yet it is necessary that we have organization of thought. But in my own subject of paleontology, I often said to the students that I would rather have them for two excursions in the field than for a whole course of lectures. So we arranged for excursions, and we established a system that I suppose the Department of Pedagogy would say was bad--of what I called the little theses. Every student on one of those trips was expected to select a subject for a paper. He was to pick out the subject himself, find the thing in which he was interested, and write it up. After a number of years I went over these theses (several hundred by that time) and I discovered that two percent of them contained things that were new to science; and before I left the University, five percent of them contained things that were new to science. In other words, I believed in the method of having them investigate something; to develop the research spirit, and find out how it is used. Does that answer your question?

DR. SEEGER: Yes, sir.

DR. MERRIAM: At one time when the little theses were being selected, a very famous student selected as his thesis--he was a specialist in chemistry--the writing up of the composition of the fat of an extinct mammoth body found in the frozen Pleistocene deposits of Alaska. The animal had been frozen in that mud for some tens of thousands of years. The question was, had the fat changed particularly in that time? I wanted to know. I had tasted some of it and wondered whether it was really fat or whether it had changed to something else. The student took a small piece over to the laboratory and laid it on his desk, and a mouse came and ate part of it. They proved that it was a mouse, by the marks of the teeth, which was an investigation itself. Word got around the class that a mouse had eaten a piece of this fat and it must therefore have been fairly good fat. That had a great effect--the fat was something edible. So here were three pieces of investigation: one, on the chemical composition; next, that it was a mouse that ate it, because of the marks of his teeth; and, third, it must have been edible.

DR. A. G. McCALL: I should like to ask, Dr. Merriam, if the same method of approach can be used for social problems as for physical problems?

DR. MERRIAM: That is a real question. I would answer yes and no. Yes, that the scientific method of approach fundamentally is the securing of accurate data as to what situations are, in other words, the facts. There is only too much social research which is only what someone thinks about it at the moment. What I wish to see in social research is very careful intensive investigation that will give us what the newspapers sometimes call "the true facts," as if there were several kinds. Social science needs to slow up a little.

To show that I am really generous with reference to that field, I feel it should have more time and more money and more effort and better organization put into it than has thus far been available.

Some years ago in Europe, I studied a group of reptiles known as ichthyosaurs, or fish lizards, which are common in some of the older formations. They look like fish, their limbs are paddles, and their tails are like the tails of fish. They have doubly concave vertebrae and other fish characters. When a famous English anatomist studied them, he thought they might be descended directly from the fishes, and therefore that the reptiles came directly from the fishes. Others had the idea that they came by another route. Then I saw a specimen in Northern Italy from a formation older than the rocks in which the typical ichthyosaurs were found. It appeared that the thigh bone, instead of being a little plate-like bone shortened up from a leg like that of the crocodile, was a long bone. I considered the idea that the farther back you go the longer this bone is, until you reach a stage like that of the crocodile.

Then I came back to America and studied the formations in the western United States for many years, and obtained some carloads of ichthyosaurs from much older formations and found that the older or earlier animals were more like the crocodile and even the vertebrae became flatter at the ends, until in the earliest known forms they were almost flat, with hardly any or only a small concavity, instead of with a fish-like biconcave form.

The study of the evolution of this group of reptiles, I sometimes call planning backwards, or looking back into the steps of the development of the thing, showing that the farther back you go the simpler it was. When we finished, we had proved that the theory was correct. In other words, the animal probably had started from a shore form and gradually changed and changed until it was adapted for life on the high seas, and rarely came out on the shore. The eggs were retained in the body of the mother and the young were hatched there. We have fossil ichthyosaurs with the eggs inside with the young ones developing. That was a long, long story but I believe that it was worth while. It is perhaps a better evolutionary story than the famous evolution of the horse, and was worth the effort put into it.

Now you will not often find a piece of investigation in the social sciences into which as much effort has been put as was used in this long study of the ichthyosaurs. But I think that the problems in the social studies would warrant putting a hundred or a thousand times as much effort on them as in the research just described. That is what I should like to see, a careful selection of social problems with more money and more time and more effort put upon them to obtain the so-called "true facts." I hope I am answering your question.

DR. McCALL: I think you are answering it very well.

DR. MERRIAM: The method of biology is not exactly the same as the method of atomic physics. There are certain things in common but there are many differences between certain aspects of research in atomic physics and in the study of some aspects of soil chemistry. Likewise there are differences between research in physical sciences and research on social problems, considered broadly; so that is the yes and the no of the answer. Does that answer the question?

DR. McCALL: I think so. I just wanted to hear your opinion and bring out the discussion on it.

DR. MERRIAM: If I had another life to live I think that, having cut my teeth in paleontology and written some general articles on these subjects, I should like to put the other life on social sciences.

DR. DEMING: While there are different methods, don't you think that the scientific attitude is really one general approach that could be applied to all branches of learning?

DR. MERRIAM: I am glad that you bring that out, because I like personally to use the term "attitudes of mind" with reference to all these things. When you say "method," you almost always wish to tie it down by sharply defined lines or figures. Attitude of mind is, I think the best term to use. If you use it for science, and art, and in some measure for religion, and so forth, you save yourself a lot of trouble.

QUESTION: I have in mind the question of social problems, specificially, certain problems that have come before the Parent-Teachers' Association meetings. There are scientists in that association and also lawyers, businessmen, and so on.

When it comes to a problem, what shall we do, and the scientist says, "Let's try this," but some of the businessmen and others -- and I find they have had the general scientific courses you mention -- they have the solution already; they say it won't work. They are conservative and we all know the conservatism of farmers, for example, toward new ideas, toward trying out something he knows in advance is no good. In other words, I would say he doesn't have the scientific attitude of mind.

DR. MERRIAM: The scientific attitude, of course, requires open-mindedness.*

DR. BELL: Dr. Merriam, I should like to ask how in this synthesis of the various fields of thought (which I take it would lead to generalizations), we are to keep those generalizations alive, moving to meet the progress in the various fields?

DR. MERRIAM: If the generalizations are actual expressions of what exists, they will immediately give an explanation to situations that have, up to that time, not been quite clear; and you will have originating from the application of the generalizations a number of new fields of work of a special type, so that the generalization over all will have lying under it a number of new special kinds of things which I suppose would themselves help to keep the generalization itself alive.

DR. BELL: The human mind tends to become stagnant on questions that are settled. I think that is one of the things that every scientific man has to fight in his own field, and if generalization does not take place as a natural result of the progress of science, I see certain dangers in those generalizations. We tend to become dogmatic. In my own mind, in all these things there is an attitude of mind to be considered. I think it is a matter of education, and that the synthesis of ideas tends to take care of itself.

DR. MERRIAM: I should say, by the way, that I hadn't thought of the synthesis as being forced. I had assumed that it would be a natural development; that is to say, as your work proceeds you perceive that certain things are related and their generalization comes into being. Newton, in his discovery of the law of gravitation, didn't force it. And so the discovery of most of these things is a more or less spontaneous thing. It keeps alive because it is true. If it is not the truth it ought to die. If it is the truth it will remain alive. The most dangerous thing that one can do to anything that is supposed to be the truth is to force it. I don't know whether that meets your idea.

DR. BELL: I think it does. Conscious effort is required to synthesize our various fields of knowledge, and that conscious effort is justified if we are constantly investigating that synthesis.

* More of Dr. Merriam's views on this subject may be had from his collected papers in volume 4 of publications 500 of The Carnegie Institution of Washington, recently reprinted. Ed.

DR. MERRIAM: So far as conscious effort is concerned, the only thing I have in mind is that if you were working in a field of considerable size and you specialized in a good many things here and there, and you obtained new results, each one of them adding to the pile of things you hadn't known before, after a while you would say, "Here are all these things we have found out in the last twenty-five years; I wonder, if we laid them all out on the table and examined them, whether some principles wouldn't come into view that we had not seen before; and if we found those principles, whether there would not be some new discoveries made that would be important to science and mankind." And so you review the whole subject to see what generalizations may be obtained. That is all I had in mind with respect to synthesis--it ought to be under continuous examination.

In my last annual report as President of the Carnegie Institution,* I wrote a few pages on this subject, but from another angle. The statement concerned interpretation of science, suggesting that as specialization goes forward, the terminology becomes so difficult that very soon, like the people who were building the Tower of Babel, you reach to a place where the men in the same large building do not know at one end of the structure what those at the other end of the building are doing. My suggestion was that if the work of those people is put into some generally intelligible form, and made available to the public, the other scientific workers could read it and understand.

In the Carnegie Institution we established a system of releases quite a number of years ago. We thought we were doing a great thing for the man in the street who otherwise would not know what was going on in (e.g.) atomic physics. Where did we get the first reaction? Not from the man in the street, but from men in other scientific fields, who said "Now for the first time we know what those people over there are doing." That was quite a discovery, and I made the point that the interpretation of the results of science was extremely important for the advancement of science and the production of the materials that would lead to major generalizations.

* Report of the President of the Carnegie Institution of Washington for the year ending Oct. 31, 1938; page 3 in particular. See also the report for the year ending Oct. 31, 1936; page 9 in particular. Ed.

DR SEEGER: Dr. Merriam, I want to go back again to that question of integration, particularly with reference to something you said last evening* about the relationship of science and philosophy.* To state my thought I should like to give just a little background. A few years ago I was giving a course in physical science in which I invited the professor of history to come in and tell what the relation of history was to physical science. He defined history as the study of man in relation to his environment. I thought that was pretty good. Later on a professor of philosphy came in to tell about the relationship of philosophy to physical science. He said, "Students, what is physical science? Physical science is the study of the relation of man to his environment." I got the point that they were all working on the same problem but approaching it from different angles As we approach the same problem from different angles, we all arrive at different questions, what is real, what is true, what is known. I suppose the general philosopher is the man that proposes to answer those questions in a general way, whereas the scientist may attempt to answer them from his own particular angle. My question is this: should the philosophy be developed primarily from a scientist answering those questions from his own angle or from the general integration, the philosopher trying to answer the questions in a general way? Of course I appreciate that both would be necessary in the long run, but is the immediate result to be obtained from the scientist or from the philosopher?

DR. MERRIAM: I should want to try it both ways, but I should think that the scientist ought to take the method of the philosopher and apply it to his own subject, and then he should go out to see what would happen if he looked at his own field from the same position and with the attitude of mind of the philosopher.

DR. SCHWARTZ: Dr. Merriam, you not only have had wide experience as an educator but in guiding the work of specialists. This group of men in the harness have passed through their university careers and are trying to handle their jobs in the various scientific fields. What is your suggestion concerning ways and means of keeping that broad outlook and still doing the specialist's job that these men are engaged to do?

DR. MERRIAM: That is almost the hardest question of all. I should say, first of all, by doing just what you are doing here at these conferences; second, that you should, from time to time, go over into the other fields and see what there is that touches your subjects. I suppose I ran some risks yesterday evening in referring, for example, to religion,** and referring to religion again under the head of the imprint of nature, but I did it quite deliberately because I have thought about it for a long time and I think it has significance. Religion is a thing that in these days often comes to be looked upon either as outmoded or as something that is put over in a separate category that does not concern particularly the point of view of the scientist, and unfortunately there are only too many people who are concerned especially with religion who do not seem to feel that there is much in science that might be of interest to them. I believe, however, that there are enough interconnecting threads so that it is worth while to discuss the two, particularly as religion has a very important influence in

*Page 27. **Pages 28 and 32.

the world. I consider it worth while to look there to see what can be of importance to the one who is thinking about the influence of science in the kind of a world in which we are living.

Now, if I may shift over just a moment to the aspect of nature that I referred to, I have been very much discouraged from time to time when I realized that the average person goes through life without seeming to notice the world of nature in which he lives, and the possibilities of things that it might give to him in the sense of enjoyment,* comfort, and strength.

I do not know whether any of you have seen a book by Dr. Beach of the University of Minnesota, called "The Concept of Nature in Nineteenth Century English Poetry," in which he points out how Tennyson and Shelley and other writers had a marvelous conception of nature in the 19th century English poetry. Then he comes to the 20th century and drops into the depths of discouragement.

There is a case, by the way, in which the concepts and broad views of what we find about us are important. There are relatively few who look far enough into nature with the spectacles of science, the telescope, the microscope and the spectroscope, and so on; very few who ever see what a magnificent universe this is, and how it is built and what effect it has upon us. I feel science has a great message to give.

I will end this with a statement that I made last night; that you are aiming toward democracy. I am strongly in favor of a scheme that has somewhere in its constitution the idea of looking toward the meaning of democracy. I am more than ever convinced that there must be continuing review of what relates to democracy, based upon the idea that such a government can succeed only as it rests upon a well educated altruistic citizenry, which ought to be educated in the field of social science as well as natural science, and which should understand art and religion.

DR. BLAISDELL: This is a good discussion and the proof of it is that I haven't felt very much impelled to interfere at any moment or felt impelled to direct it along any particular line. But we have as a guest today Dr. Atwood from the University of Florida, who is engaged in a work that derives a good deal of its inspiration from association and contact with Dr. Merriam, particularly in this field of integrating the various fields of science at the undergraduate level.

Dr. Atwood, we should be glad if you would take a few minutes to tell us what you are trying to do in Florida in that connection.

DR. ATWOOD: At the University of Florida we have attempted to put some sort of integration into effect. You are all familiar with the problem that faces state universities, where large numbers of students enter and at least fifty percent of them have to leave for one reason or another by the end of the second

*The reader may recall the experience related on p. 30.

year. While sitting around the table wondering what those students had when they left the University of Florida at the end of two years, we became very discouraged. They had two or three spoonfuls of pre-doctor's degree chemistry, two or three spoonfuls of pre-doctor's degree physics, two or three spoonfuls of pre-doctor's degree English history, and so on and so on. We decided to set up a two-year period of general education that would net something worth while to those students who stayed only two years, and at the same time would be satisfactory as preliminary work for upper division or regular college and university work.

We set up six basic courses. We set up Man and the Social World; Man and the Physical World; Man and the Biological World; English (called Reading, Writing, and Speaking); Man and His Thinking; and a course in Humanities. Those six courses formed the foundation of a general educational program. They all meet four hours a week right through the whole year, with the regular six hour objective comprehensive examinations at the end.

Four of those courses: Man and the Social World, Man and the Physical World, Man and His Thinking, and English, come in the first year. Man and the Biological World, and the Humanities, come in the second year. The problem is twofold--to set up a program that is satisfactory for the two-year student, and at the same time give a satisfactory preparation for the four-yearsstudents.

In the second year we have what we call terminal electives for the two-year students, and connective courses for those that intend to go on further. As you would expect, the connective courses are a bit harder and go into things deeper than the general electives. So the two-year student comes through this basic program, gets his Associate of Arts degree, leaves the University and goes merrily on his way. The ones who have shown ability in the basic courses and are planning to go into the upper division go into the connective courses, which bridge the gap between the broad basic course and the third-year courses in the various fields.

In regard to the problem of integration, we tried all the various methods of doing it on paper and even called in some professional pedagogues to help us, and finally decided the way to do it was to get the man who was teaching to teach at several levels. For example, the fourteen men teaching Man in the Social World are divided up more or less evenly between those four fields, so that the man who is teaching the basic course is either chairman of the connective course or is actually teaching it himself or teaching part of it. The same thing goes on into the junior or senior year. In some cases we have men teaching courses in the Graduate school, in the upper division, in the connective period, and in the basic freshman course. The surprising thing is that these men up here who are supposed to be the most outstanding men in the University are the ones now anxious to teach in the basic courses. It is a very interesting thing and of course exactly what we hoped to develop.

Now just one minute on the course Man and the Social World. I can describe it by saying that we use the social sciences, we don't teach them. In other words, we use history, political science, sociology, and economics. We use all fields but we are teaching Man and the Social World. Man is a part of the social

world, influenced by and using the physical world to get the goods and services necessary to satisfy his needs and wants. This concept is the theme that runs through the whole year's work, and the student has got this continually in front of him and his generalizations are related to this theme, not to history or to any part of it.

We have two lectures a week and two discussion groups a week. Each student goes four times a week; two big lecture sections, where the purpose is to teach concepts, ideas, or ideals; then he goes into small groups of twenty-five or thirty twice a week where these things are discussed.

About two or three weeks ago the lecturers were dealing with civil, economic, and political rights. The student says "Well, what are these rights? What about this watch of mine? Is it mine? Why is it? Why do you say it is yours?" Finally the student says, "Because I can give it away; I can sell it. Why do you call it private property?" Pretty soon he says "Well, I can pass it on. I can destroy it if I want to." "How about your necktie, is that your private property?" "Well, I have the right to do this or the other thing with it. But have you the right to hang somebody with it?" and he says "No." Well, you are right down to the basic principles behind the concepts that are in the lectures, coming down to everyday examples in the students' own lives, and realizing, of course, that private property is an institution, as the social scientist is bound to have it called. If it is an institution, what is it based on?

Now while jumping from one thing to another, I have tried to show you graphically what the program is and a little regarding how it works. It is an attempt to carry out in a broad way with an undergraduate program the ideals and concepts that Dr. Merriam spoke of last night and this morning. I want to make it perfectly clear that this is not an official description and the opinions expressed are definitely my own reactions to the program.

MR. TOLLEY: How do your students react to this?

DR. ATWOOD: Well, the first year, with three classes of conservative old-plan students, the freshmen were more or less high-pressured into not believing in it, but by the time there were three classes at the University who had been through the new program, then you began to see seniors who hadn't attended those classes coming back to visit these lectures. They said: "What is this thing we have been hearing?" "We are coming back just for the he qq of it, to find out what it is all about." This year the first class graduates that has gone through the whole program. I don't want the whole program judged by this first class, however. I want to wait two more years before we judge the program, but the reaction has definitely changed as the students go through it.

MR. TOLLEY: How much have the contents of the course changed out of the experience of the five years?

DR. ATWOOD: Very much. We started out by filling these courses so full of everything we knew that nobody could ever have taught the course, and if he did,

nobody could have gotten anything out of it. We try hard to choose only the material that will help to put the concept across. We like to have some reason why factual material should be included. We are coming out with about twenty or twenty-five basic concepts. That means there is about one concept a week so far as the students are concerned. The thing we should like most at present is to have more discussion groups to back up the lectures.

DR. BLAISDELL: Dr. Merriam, have you any comments you would like to make on Dr. Atwood's presentation?

DR. MERRIAM: No, I think it is a very interesting scheme to work out in a University. I have talked to Dr. Atwood a little bit about it and I talked on this subject some years ago when I was down there at Gainesville.

DR. BLAISDELL: I am sure I have a number of questions that I would like to put to Dr. Merriam growing out of the talk last night. There are probably a good many here who feel that we have just gotten well into the field that was covered so well by Dr. Merriam, but our deadline is rapidly approaching so I think this is a good time to stop.

Ten Lectures and Discussions on

SCIENCE: Its History, Philosophy,

and Relation to Democracy

held at the Department of
Agriculture Graduate School
under the chairmanship of

M. L. Wilson
Under Secretary of Agriculture

Edited by W. Edwards Deming
Assistant Director of the Graduate School

Lecture III

DOES SCIENCE AFFORD A BASIS FOR ETHICS?

by

Edwin G. Conklin

The Graduate School
The Department of Agriculture
Washington
1939

Lecture III

DOES SCIENCE AFFORD A BASIS FOR ETHICS?

by Edwin G. Conklin
Emeritus Professor of Biology, Princeton University

In the auditorium at 5 o'clock
22d March 1939

I

I suppose that I have been asked to speak upon this subject because I discussed it a year ago in an address before the American Association for the Advancement of Science. This, however, isn't the address that I gave on that occasion. The subject of ethics is long and complicated. Many, many books have been written on it, and I have only another phase of it to take up with you this afternoon.

No doubt the question in the mind of Under Secretary Wilson who suggested the subject to me was the fact that science has undermined so much of the traditional religion on which ethics was largely based in former generations. We have all felt this; everyone of us must have felt it and felt keenly the lack of a certain social morale that has been caused by this undermining of religion and the sanctions of religion. People are no longer frightened by threats of hell nor lured by promises of heaven, and the question is, what basis is left for ethics? Many people believe that science has debased man and destroyed spiritual values. It has shown that the world in which we live is a tiny speck in a second-rate solar system, that man is only one of a million species of living things that exist on the earth, and that he is of little significance in the vast system of nature. This feeling is rather widespread and it has largely influenced our concept of ethics.

In attempting to answer the question, "Does science afford a basis for ethics?" It will be desirable to deal first with the theoretical and then with the much more serious problem of practical ethics. Is ethics natural or supernatural in origin? That is the key question. When I was a student in college we were taught the supernatural origin of man. Mind, language, and especially ethics, were gifts of God by which man was sharply distinguished from all other animals. A few years ago the students at Princeton University staged a debate between myself and a professor of a theological seminary on the subject of evolution. It was at the time when William Jennings Bryan was stirring up the country on the dangers of evolution. And let me tell you there was a good deal of keen sense on the part of Mr. Bryan in this, for he recognized that the doctrine of human evolution was striking right at the traditional view that man was above

nature, whereas evolution was teaching that man was a part of nature, and that he had come into existence by natural processes.

Well, in this debate which the students had staged between me and the theological professor, I spoke first on the evidences of organic evolution and that of man in particular, but when my theological colleague rose to speak he surprised me and the audience by saying that he had no objections to offer to my argument; he was willing to let the biologists deal with the origin of man up to the time when man became a free moral agent. There, in his belief, natural evolution ended and supernatural causes entered. This is the position of many liberal theologians today, but while their recognition of the evolution of lower forms of life and of the body of man is gratefully acknowledged by the scientists, the final introduction of the supernatural does not satisfy the naturalist. To the thorough-going scientist "Nature is everything that is," and the evolutionist finds abundant evidences of the natural origin of mind, language, and even of ethics. Indeed, even the supernaturalists in these evil days of crises attribute the ethics of some modern peoples and nations to the Devil rather than to God, whereas the naturalists, if they are genuine naturalists, ascribe both good and bad to nature.

There is positive evidence, as you all know, that in times long past there were types of human and partly human beings that were more brutish in body, mind, and social relations, than the general average of the present races. There is abundant evidence that ethics has undergone evolution no less than intelligence. It has developed from its beginnings in a primitive family group through tribal, racial, national and international relations, from the ideals and practices of savagery to those of barbarism and civilization, from the reign of vengeance and retribution, as shown in the ancient code "an eye for an eye," "a tooth for a tooth," and "whosoever shedeth man's blood by man shall his blood be shed." This was the iron rule of retribution. Ethics has proceeded from this to that highest conception of ethics embodied in the Golden Rule. But just as in physical evolution, there are retarded or retrogressive individuals or races, so also in the development of ethical ideas some people and periods are far behind others and all fall short of their highest ideals.

In this question of the origin of the mental and moral characteristics of man it is not necessary to go back into the distant past of evolution for we can see in children today the development of the mind, the acquirement of language, and the growth of ethical ideals. Nowhere in this development is there a sudden introduction of supernatural factors. If anything in the world is natural, the development of the body, mind, and morals of a child is natural. This is not to say that we understand all about nature, that there are no mysteries involved --

indeed the more we know of nature, the more mysterious it becomes. For all science knows to the contrary there may be in the whole of nature, from sands to stars, from germ cells to geniuses, a mental, moral, teleological, ethical substratum or medium in which all things exist and develop: but, if such there be, it also is a part of nature and not some foreign supernatural interference with the regular processes of nature.

II

Those who hold that nature cannot accomplish what we see around us and feel within us have too poor an opinion of nature. They forget or are ignorant of the marvels of development. They contrast the most highly developed type of intellect, ethics, and human personality with the lower forms of life or with inanimate matter and declare that "never the twain shall meet." Looking at the end product of any development it does seem incredible that it should issue from such simple beginnings, that a beautiful flower or butterfly or bird should have come from a germ cell. If we did not know that it is true, it would be incredible that a microscopic egg cell could develop into an elephant or a man. Or, most wonderful of all, that geniuses like Socrates, Plato, Aristotle, Newton, Darwin, Pasteur, Shakespeare, Goethe, Beethoven, were once babies, embryos, and germ cells: and yet no one denies this. It does seem incredible that reason, emotion, aspiration, and ethics, should develop out of such simple functions and processes as sensitivity, reflexes, trial and error, and yet these incredible things are actual facts that can be verified by anyone who will take the trouble to investigate them.

Similarly, it seems incredible that all the hosts of heaven and the furniture of earth should have come into existence by a process of natural development or evolution, and yet almost universally scientists hold this view, and more and more people accept it. The evolution of galaxies and stars and solar systems; of the earth with its oceans and mountains, plains and rivers; of plants and animals and man himself; the evolution of all the chemical compounds found in nature, and even the evolution of the chemical elements are plainly indicated by all the evidences available. In short, evolution, like individual development, is a universal law of nature. There is no creation out of nothing; <u>ex nihilo nihil fit</u>.

Everything in the universe comes into existence by transformation rather than by new formation. And yet in this process of transformation or evolution new properties appear as the result of new combinations of the same old elements: for example, new combinations of protons and electrons give rise to new atoms; new combinations of atoms to new molecules; new combinations of molecules to new compounds. In a similar manner in the living world new combinations of genes give rise to new

chromosomes, and new chromosomes give rise to new types of organisms, so that we have this production of newness by the simple process of new combinations--what has frequently been called creative synthesis or creative evolution, or, more recently, "emergence." In the development of a universe, no less than of a man, new forms and functions appear as a result of these new combinations.

Psychical development begins with differential sensitivity, or the ability to respond differently to stimuli of different quality or degree. For example, suppose you take a single celled protozoan like a paramecium, and perform the simple experiment that is often used to show students the behavior of such an organism. You put them in a trough of water, one end cooled by ice, the other heated by a flame, and you find that in a short time they are all gathered in the middle region. As you watch any one of them you find that as it gets to the hot end it slows up, pretty soon stops and backs, and finally turns around by a series of movements and goes in the other direction until it gets to a region which, we will say, is uncomfortable (I don't know why we should deny that feeling to the paramecium), when it backs again; and so it keeps going back and forth, avoiding extremes of heat and cold and keeping in a middle region which is comfortable.

Plants do similar things. If you take a germinating bean seed, as we do in elementary courses in biology, and pin it on to a cork sheet which is held in a vertical position and kept wet by water, you find that the shoot starts to grow up and the root to grow down. Then if you rotate the sheet through 90 to 180 degrees you will find that the shoot will continue to turn up and the root down until it will wind up into a spiral like a watch spring. These parts of plants are doing what the animals do, they are responding differently to different kinds of stimuli, or to the same stiumlus when it is in different degrees, and they are responding in generally beneficial ways; they are moving in ways that are satisfactory and avoiding those that are unsatisfactory.

Now such behavior is fundamental in all living things. They follow or grow toward the satisfactory and avoid the unsatisfactory. Here are the beginnings, the very elements of the psychic life: and it is evidently based upon the ability to distinguish or differentiate between that which is satisfactory and that which is not, and to follow after the one and avoid the other. And this very fact, this very principle of distinguishing or differentiating and choosing or following is the basis of wisdom in men as well as in animals and plants. There is indeed a wisdom of animals and plants that is based upon the same fundamental principles as in the wisdom of human beings.

In the psychic development of higher animals and man differential sensitivity gives rise to the special senses. Tropisms or reflexes, which

are fixed methods of response to stimuli, are linked together into instincts. Conditioned responses that are often repeated become habits. Effects of responses that are stored in the protoplasm are known as protoplasmic or organic memory, such as are seen in the training of muscles and nerves in learning to walk or talk or play games. When one protoplasmic memory comes to be often associated with another and different one we have associative memory. Any animal that can learn anything, as for example the association of the sound of a bell with the presence of food, has associative memory. Such memory is found only in animals with a nervous system. Each of these steps in psychic development is an "emergence" to a new and higher level, and each new level makes possible further development to still higher levels.

III

With increasing complexity of stimuli and responses, behavior becomes less rigidly fixed and is more variable. Every biologist who has tried to demonstrate to a class what should happen under certain conditions is sometimes much disturbed because it doesn't happen. Animals do not always behave as they "ought" to behave. Their behavior shows some variability or modifiability, and is not fixed. Recently the physicists have been telling us that even in physics things are not rigidly fixed. Charles Darwin of Cambridge, physicist, and the grandson of the great evolutionist, said in his presidential address before the British Association last August,* that there is a certain fuzziness about all phenomena in nature -- things are not absolutely clear-cut and sharp, but there is a certain variability. Now this is especially true of living things and this brings in the possibility of modifying behavior. Instead of responding to stimuli in a purely mechanistic way, organisms seem to have a certain amount of freedom, which may be due to conflicting stimuli or internal states or previous experience.

I can make this plain by telling the story of a bull frog which we kept in our laboratory for several years. When we first brought him in he was frightened whenever anyone came near his aquarium, and he would go off in a dark corner and hide. By gentle treatment and by holding up buzzing flies on forceps, we could induce him to come forward and grab the fly, after which he would go back once more. You could actually see in the behavior of this frog the balancing of opposing stimuli. The stimulus of hunger lead him to come forward; fear would cause him to retreat. Finally when he found no harm in coming forward he would come to the front of the aquarium whenever anyone came near. He had learned to control his fear, and there had ceased to be any inhibition in his responses to the stimulus of hunger.

* C. G. Darwin, "Logic and probability in physics", Nature, vol. 142, 27 August 1938.

In this way a certain amount of freedom comes about, and freedom is invariably measured by the extent to which remembered experience influences behavior. Remembered experience is what we call intelligence, or rather, intelligence is this capacity of profiting by remembering experiences. Reasoning is only intelligence of a higher sort, where we deal with things in general rather than with specific instances. Let me illustrate this distinction between intelligence and reason. On the farm where I was brought up we used to have a horse that had learned the trick of opening a gate. He knew just how to do that; he didn't make any mistakes once he had learned. He was just as intelligent as I was in opening that gate. He had learned by experience to lift the latch and let himself out. But when we put another kind of a fastener on the gate, he was lost: he couldn't reflect as a man could, "Now this after all is a mechanism for fastening the gate and it must have certain qualities similar to the old latch that I am acquainted with." He could not do that; he had to learn it all anew. He was not able to generalize. Reasoning is the power of generalizing, of comparing things and seeing certain resemblances that are fundamental, and ruling out those things that are accidental. Reasoning develops in the life of the human individual from these simple beginnings that I have been describing.

Of course, there are many other things that go into psychical development that I haven't time to describe, such as food, vitamins, hormones, etc. You all know that in the building of any structure there must be materials such as bricks and mortar. Likewise, in the building of the body and mind there have to be carbohydrates, fats, proteins, enzymes, hormones, and vitamins, and they play a very important part, but they are only the bricks and mortar that are used by the particular form of protoplasms and cells that are building the body and all its functions.

VI

In general, development is a gradual process, but we recognize that there are stages when it passes from a lower level to a higher level by the process of emergence. Finally, the highest level of human development is attained when purpose and freedom, joined to social emotions, training and habits, shape behavior not only for personal but also for social satisfactions, for society no less than the individual is seeking satisfactions, and when all of these things combine, we have what we call ethics, or the science of right conduct. Thus ethics is born and man becomes a free moral agent--not absolutely free, of course, nor absolutely moral, but an agent of limited capacity and responsibility, who has developed under natural laws from a condition which is neither free nor moral nor responsible.

Since ethics depends upon training and habits as well as upon heredity and development, its approved codes vary from time to time and among different peoples and races. What is regarded as a high type of ethics in one race or age is wholly condemned in another. And in general there has been throughout the course of human history an evolution of ethics from relatively simple and crude and local types to more complex and refined and universal ones.

Here, in brief, is what may be called the scientific idea of the origin of man--of his body, mind and morals--and it is in sharp contrast to the traditional view of the supernatural origin of all of these. Many persons, even many scientists, have assumed that since all this development is the result of natural processes it has degraded man, debased reason, destroyed freedom, debunked ethics, and last of all, eliminated God. In his condemnation of evolution old Thomas Carlyle said: "I have known three generations of Darwins, atheists all." It is interesting, that he knew Erasmus Darwin, the grandfather of Charles Darwin, Robert Darwin, the father, and finally, Charles Darwin himself, the great evolutionist. If you will read the "Origin of Species" by Charles Darwin, you will see that in the last paragraph of that book he says, "There is grandeur in this view of life, with its several powers, having been originally breathed by the Creator into a few forms or into one." I merely bring that out to show that after all Charles Darwin wasn't necessarily an atheist, in spite of Thomas Carlyle, who continued: "Ah! it is a sad and terrible thing to see a whole generation of men and women wandering about in a purblind way and finding no God in this universe. And this is what we have got, all things from frog spawn; the gospel of dirt the order of the day." Carlyle didn't know much about frog spawn and the wonders of development. Apparently he didn't realize that the greatest human geniuses have developed from germ cells. Evolution is no more atheistic than individual development, and certainly neither of these has destroyed human freedom, responsibility, nor ethics.

Darwinism, which is the doctrine of natural selection, or the survival of the fit, has been supposed by many people to be adverse to ethics. Bernard Shaw said that if Darwinism were true, only fools and knaves could bear to live. The survival of the fit and the elimination of the unfit meant to him, as it meant to Nietzche, Bernhardi, and militarists in general, the survival of the strong, the ruthless, the nations that are feared rather than loved; and conversely, the elimination of the meek, sympathetic and peace loving. But this is a total misconception of Darwinism. Darwin was too wise a man to lend his support to any such extension of his principle to the social and moral fields. He knew, of course, that there are many kinds of fitness in human beings. There is not only the fitness of physical strength to

be considered but also the fitness of mind and of moral and social conditions, and Darwin said it is altogether wrong to attempt to force upon moral and social condtitions the same principles that apply to physical conditions. Physically, the fittest are those that are most capable of living and leaving offspring. Sometimes they are strong and ferocious and sometimes they are mere parasites. Mentally, the fittest are the most rational; socially, the fittest are the most ethical. To measure social relations or ethics by mere brute force and strength is to miss the whole fact that man has had a three-fold evolution. There has been an evolution of his body, an evolution of his mind, and an evolution of society and his social relations; and while these principles are always balanced one against another and sometimes are apparently in conflict, nevertheless they do generally cooperate and bring about important advances along the whole line. We can't possibly subscribe to any doctrine that would merely bring about physical development of a human being and at the same time subordinate his intellectual or his social development.

In conclusion, evolution does not destroy the dignity of man. His real dignity does not depend upon the method of his origin; it does not depend upon the fact that he was once a germ cell and then an embryo and then an infant. It depends upon what he is capable of becoming, the possibilities of his development. There is where the real dignity of man is found. This is what makes him more dignified than the dog or the horse or the plant; he is capable of going farther in his development than these other living creatures.

Likewise, the importance of ethics is not to be measured by its humble beginnings nor by its present defects but by its possible developments and its influence upon human welfare. In all men, fundamental ideals of right conduct are much the same, but in practice these ideals are too narrowly limited. For example, the ideals of right conduct toward individuals who are closest to you, e. g. toward your children, are very much the same with all peoples of all races. Among some savage tribes there is no ethics or altruism that extends outside of the tribe: every tribe for itself and the devil take the others. Their altruism does not go further than that. As you go up through higher and higher social grades you find that altruism reaches farther and takes in more people, until with some people, altruism includes the whole human race. We feel sympathy for the people who are suffering in Spain and in China. We feel resentment toward those who are making them suffer, because they are dealing with ethics in a small and local way instead of the larger and more general way toward which evolution is leading.

Science then affords a sound intellectual basis for ethics. The ethics of science does not differ from that of the most enlightened religions. Whether written on tables of stone or in human nature itself the cardinal virtues are still virtues and the deadly sins are

still sins. The ethics of science regards the search for truth as one of the highest duties of man; it regards noble human character as the finest product of evolution; it considers the service of all mankind as the universal good; it teaches that both human nature and humane nurture may be improved, that reason may replace unreason, cooperation supplement competition, and that the progress of the human race through future ages will be promoted by intelligence and good will.

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All that I have been saying is of the general sort that might be called theoretical ethics. When we come to practical ethics we enter a veritable jungle of thoughts, opinions, and beliefs, not founded upon real scientific and verifiable evidence but upon our inclinations to think this, that, and the other. Following the example of the present alphabetical practice in which initial letters are used instead of entire names, Henshaw Ward has called such thoughts, opinions, and beliefs, "THOBS".

Practical ethics applies to every social relation from the sexual relations of men and women and the relations of parents and children to those between employers and employees, between capital and labor, between producers and consumers, between individuals and society, between classes and races and nations. In spite of extreme differences in ideals of good conduct in all of these relations, there is everywhere a desire to promote better conditions for one or the other of these parties, and the desire for the better social relations in general is widespread among all people. We must give credit to the Nazis, the Fascists, and the Bolshevists for desiring to bring about better conditions, even though we may feel, as I do, that they are profoundly mistaken in the ways in which they are going about it.

The faults and failures of present social relations are almost universally recognized. Revolutionists desire

> "To shatter this sorry scheme of things to bits
> And mould it nearer to the heart's desire."

But generally the way of evolution rather than that of revolution is the way of progress. Conflicts between democracy and autocracy, between anarchism and absolutism, between Communism and Fascism are conflicts of ideals in attaining the good society, and as such are ethical in aim, however mistaken they may be in practice. It may seem incredible that hate, intolerance, murder, war should be employed to promote ethics, but the contestants always proclaim, and probably sincerely, their high ethical ideals. They always claim to have truth, right, justice, even God, on their side, although they may employ the most inhuman methods in attaining their aims.

Practical ethics is thus a jungle of conflicting emotions, ideals, practices, usually founded upon "THOBS" rather than upon scientific evidence. In an attempt to break paths through this jungle, innumerable ethical codes have been set up which are almost as varied as human relations themselves. To promote justice and harmony among all these conflicting groups, governments are established among men and general codes are laid down in the form of laws. Our laws are codes of ethics. They are administered by courts of justice, which also deal with codes of ethics--all in the interest of better social relations. But outside these major and minor paths through the jungle there are multitudes of social relations which are not touched by codes or laws. Probably no cause of social discord is equal to that of differing standards of ethics. Violations of our personal ideals of honor and right conduct create resentment and anger. Ethics or its violations fill our newspapers, occupy our legislatures and courts, concern our churches, distract our homes. It was Matthew Arnold, I think, who said "Conduct is two-thirds of life." It is even more than that, it is almost all of social life.

Probably the most important ethical and religious code in history is the Decalogue of Moses, which was summarized by Jesus in the two great commandments, "Thou shalt love the Lord thy God with all thy heart, soul, and mind, and thy neighbor as thyself." If for the person of Deity there be substituted the qualities of Deity, namely, truth, justice, mercy, love, these are the commands of science as well as of religion. Likewise, the Golden Rule is the simplest and at the same time the most universally practicable rule of ethics ever proposed: "Whatsoever ye would that men should do to you, do ye even so to them." Science and religion are limited to no class or race or nation. The ethics of science as well as that of pure religion is world-wide in its extent and altruistic in aim.

The ethics of great scientists is essentially similar to that of great religious leaders. The profession of the scientist, like that of the educator or religious teacher, is essentially altruistic and should never be prostituted to unethical purposes. Unfortunately, these high ideals are no more fully realized in science than they are in religion. Scientists as well as religionists have all the frailties of human nature and both fall short of their highest ideals. It has always been true and will continue to be true that ideals are better than performance. Mark Twain said on one occasion, "To be good is noble, but to tell others to be good is noble and no trouble."

The age-long problem with which religion and ethics have struggled is this, how can men be induced to live up to the best they know? How can they be brought to substitute the spirit of service for selfishness, of love for hate, of reason for unreason? The long efforts of past centuries show that there is no rapid solution of this great problem, but in the cooperation of science, education, and religion, there is hope for the future.

SEMINAR

following the lecture by
Dr. Edwin G. Conklin
Office of the Under Secretary
Thursday morning at 9 o'clock, 23d March 1939

Syllabus

1. The relation of agriculture and the industries to the rest of the public is a problem in ethics. Danger lurks in the pressure of organized minorities, and some modification of our form of democracy may be required to cope with them. Pp. 60-61.

2. Our social system is in bad order. Problems of distribution cheat the world of a good share of what science has contributed, or could contribute. P. 62.

3. Protoplasmic memory, pp. 62-64. Instincts are inherited protoplasmic memory. The principle of natural selection. Pp. 65-66.

4. Adjustments of the individual to his environment. The elimination of useless responses, and the persistence of useful ones. Unusual tolerances for poisons and high temperatures can be developed. But can these adaptations be passed on? The answer is probably, no. Pp. 65-66; see also p. 63.

5. Adjustments of the race to its environment. Explanation by heredity and mutations (Morgan). The enormous complexities that must be explained; the development of the eye, for example. Pp. 66-67.

6. All of nature behaves as if directed to some purpose; the fundamental aim seems to be survival. P. 67.

7. The universe is orderly, but the more we learn of nature the bigger the mystery of the beginning of things. Pp. 69-70.

8. Democracy is the highest possible development of individuals. In a democracy, people are measured by their own worth to society. The fundamental principles are liberty, equality, and fraternity; each of these is a biological necessity for the fullest development, but each has a biological limitation. Pp. 70-72.

9. The scientific method will help to solve problems in practical ethics; p. 72.

10. Science leads us to the two great commandments of the Christian church; p. 73.

11. Both the intellect and emotions are a part of our lives. Science has dealt too much with the intellect, and not enough with the emotions. The faults of religion in this age are the reverse. Pp. 68; 74-76.

Proceedings of the Seminar

DR. BLAISDELL: Dr.Conklin, we try to have a free and frank discussion of various questions that have been raised on account of your lecture. The object is to develop those points of view so far as it is possible in a group of this kind.

DR. A. G. McCALL: Dr. Conklin, I followed your splendid presentation with a great deal of interest yesterday afternoon. I am curious to know how you apply ethics to agriculture. Since this is dominantly an agricultural group, this question would doubtless be of interest.

DR. CONKLIN: The functions of government are essentially ethical. As I indicated, I think laws have for their purpose the improvement of the relations between individuals in society. Justice is an ethical problem, and similarly with regard to the multitudinous industries and activities of the people that the government undertakes to regulate. The larger the number of these relations that have to be looked after, the more complex the government becomes and the more difficult it is to bring about really ethical relations among very great numbers of persons. Much that is going on here in Washington and in the various States is an attempt to establish ethics--proper behavior among individuals, i.e., behavior that will permit the greatest happiness and liberty to all.

Now, so far as agriculture itself is concerned, the relation of the farmer to the rest of the public is an ethical problem, just like the relations of the various industries, manufacturing, and communications of one sort or another to the rest of the public. They all come under that general heading, and all of course are supposed to and ought to deal with the welfare of the entire community. It would be unethical for any one group to get an unfair advantage over the others. It would make the majority, the larger group, suffer for the benefit of the smaller group. One of the biggest problems in all government is these pressure groups that our legislatures suffer under, where an organized minority can bring about legislation that will adversely affect a majority.

I am a thorough believer in democracy and I believe in majority rule, because democracy is founded upon that principle, and I believe, therefore, in the greater good of the greater number. Now, of course, the farming element is a very large element in our society and has held the short end of the lever; it hasn't had its share of profits. I think that can be truly said when you consider what has been done for industry and manufacturing by means of protective tariffs through so many years, during which the farmer was getting very little

help. I can well see why there should be the feeling that it is now the farmer's turn to get assistance. On the other hand, I do confess to a certain amount of alarm in seeing so many farmers, as well as so many others in the nation, depending upon the government for assistance, instead of meeting their needs as well as they can be met, without assistance, just simply lying down on the government. That, of course, is a thing we can understand. As long as the getting is good, get your share. That is a common expression and a common feeling. But it certainly is a thing that is not making for the welfare of society, and where we are heading I don't know. I am a good deal disturbed by the conditions that are generally prevailing, namely, of relying upon the government for everything. Of course, the end of that must be communism or an extreme form of socialism.

I was talking with a very eminent lawyer only a few days ago, and he said he wondered whether democracy wasn't beginning to fail, whether in a very large and extraordinarily complex society, we can give to the individual the liberty that hitherto he has had. Of course, we all know the tendency of limiting the individual. It has been going on ever since society started. My grandfather's nearest neighbor was twelve miles away, and he could do about what he pleased. He ran his own still, made his own whiskey, and he shot game all over his place. Apparently he didn't interfere with other people and they didn't interfere with him. But when people get crowded, then mutual rights must be considered.

Industry and communication have had effective leadership; agriculture in general has lacked it. Farmers are proverbially individualists, they are constitutionally democrats -- I mean democrat with a small initial letter. And so their plight has been more serious than that of the manufacturers or any other group, and the result has been that enormous fortunes have been built up in certain manufacturing industries while the farmer has suffered and feels like the toad under the harrow. It is his turn now to get some of the good things of the world. I sympathize with him; I have been a farmer myself in a small way, and I know something about the hard work of farming; and yet with all of that, I fear for the future of individual initiative and responsibility. Of course, it may be that some modified form of the democracy that we have so cherished is a necessity, and that we are inevitably going in that direction.

When it comes to the details of agricultural practice, you people here in this Department are doing the proper thing. You are helping farmers to have better crops and better flocks. You are showing how to make two blades of grass grow where one grew before. You and the colleges and universities of the country are showing by experimental breeding the very valuable results that can be gotten by following the principles of Mendelian inheritance. You are heading in

the direction of a life of greater abundance. There is no doubt that science has made life easier and more comfortable for the masses of mankind. And yet there is suffering in the midst of superabundance. The gifts of science are not impartially distributed. The more abundant life made possible by science is in conflict with the profit system, and that social system is breaking down. Anyone who can't see the signs of the times is purblind. We are surely going to see very great changes in our social system in the next fifty or one hundred years, perhaps in less time. Our present difficulties are with the inequitable distribution of the goods of the world. We need all of the goods that science can produce but we need to have them distributed in a more ethical manner.

DR. WINTERS: Dr. Conklin, in your discussion you mentioned the dogma of new characteristics, new genes producing new chromosomes, and new chromosomes new individuals.* Further on in the discussion of ethics, you mentioned protoplasmic memory.** Do you distinguish between those two forces in evolutionary development?

DR. CONKLIN: Protoplasmic memory is one of the fundamental characteristics of all protoplasm. That is, protoplasm that has once responded to a certain stimulus has been changed, or modified for a longer or shorter time. For example, there is a plant which some of you are familiar with, known as Dionaea muscipula, or the Venus's flytrap. It has a leaf where the midrib is a sort of hinge, and it can close and catch an insect. There are in each half of the leaf three sensory hairs. If you stroke those hairs once with a pencil or a camel hair brush, you get no response. But if within the space of about three minutes you stroke them a second time, the trap closes. Now what happened? You would say after the first stroke that nothing had occurred. But the second stroke shows that actually there was some change; something had happened in the protoplasm at the first stroke that made a second stroke effective in causing the closing. Sometimes we speak of this as a summation of stimuli. That is, the first stimulus needs to be supplemented before you can get the response; but whatever a summation of stimuli is, it means that some effect of the first stimulus has been left in the protoplasm. Protoplasmic memory in its simplest condition is the retention of some effect of a previous stimulus.

Similar phenomena are found everywhere in the animal kingdom. I made experiments many years ago on the length of time that newly hatched chickens would remember a previous experience. I took grains of corn meal of natural color, and others that had been stained red with a dye and to which had been added quinine, making them bitter; these were put out for the little chicks. They would pick up the grains of naturally colored corn and swallow them, and evidently enjoy them, and then pick up the red grains and spit them out. The question was, how long would they remember the difference between the two? Well,

*Top of page 52. **Page 53, end of Section II.

at first it was only for a few minutes. Gradually, they remembered the experience longer, so that ultimately they would avoid altogether the red grains and pick up only the others. Protoplasmic memory in this case became associative memory; they associated the red color with the bitter taste.

In our own case, associative memory has gone much further than this, but it grows out of protoplasmic memory. For example, our bodily activities are very many of them protoplasmic memories. We walk without the slightest thought of what muscles are to be contracted in definite sequence. Think of the complexity of talking; it is perfectly enormous when you really think of the number of muscles that have to be brought into coordinated action, and yet it goes right forward without any thought on our part. (Laughter) People can talk without thinking, and this is due to protoplasmic memory. Our habits are largely based on protoplasmic memory.

DR. WINTERS: Does that go beyond the experience of the individual, or is it confined to the life of the individual?

DR. CONKLIN: I think it is confined to the life of the individual. All the children that I have ever known had to learn to talk. They had to learn to speak even their mother tongue. If they had been brought up by Chinese nurses and had never heard the English language, they would have spoken Chinese just like a Chinese child.

DR. TUCKERMAN: Did they have to learn to nurse?

DR. CONKLIN: That is what might be called an inherited reflex; and a series of reflexes, one following another, is what is commonly known as an instinct. There are psychologists who say there aren't any instincts, but when you see animals doing things that they never saw done before and doing them perfectly, as they do, there can't be any doubt but that such actions are inherited, but a great many of our so-called instincts are really reflexes. For example, the pupillary reflex of the eye is a direct response to light. When the light is strong the pupil closes. This is undoubtedly an inherited reflex. On the other hand, the directing of both eyes to the same point is not perfectly inherited, because a new-born baby will sometimes direct one eye in one direction and the other in another. Within a few days it learns to concentrate both eyes on one point, but binocular vision is a thing that is acquired, though there are indications that it is being gradually transformed into an inherited act. I think that some acquirements do get into heredity but nothing like as often as most people who believe in extreme Lamarckism would maintain.

The most famous experiments in recent years on the inheritance of learning were those that were made by the late Dr. William Mac Dougall at Duke University and previously at Harvard. He was Professor of Psychology at both places and he trained white rats through some forty-four generations. You are all familiar with the experiment, perhaps, or shall I tell you the story? I don't want to waste your time going over things that you are thoroughly familiar with. He trained white rats to find their way out of a tank of water. There were two ramps that they could go up, one of which was illumined by an electric light, and the other was dark. If they went up the illumined one, at the top they struck a copper plate that was heavily charged with electricity, and they got a shock and were often thrown back into the water. He found that untrained rats made about one hundred and forty mistakes before they learned to always avoid the lighted ramp. After that, when they saw the lighted pathway they always took the other one and thus avoided the shock. After some forty generations, he found that the descendants of the trained rats made only about twenty-five mistakes instead of one hundred and forty, and this he attributed to the inherited effects of learning.

Other people who repeated these experiments did not get these results. Now science, in order to be science, must be verifiable. McDowell at Cold Spring Harbor, Crew at Edinburgh, and several others, repeated that work but didn't get those results. Human beings have been repeating such experiments for innumerable generations and still we have to send our children to school and still they have to learn the things that we laboriously learned. They don't seem to learn any quicker than their parents did. And it does seem out of line with human experience to maintain that learning is inherited.

DR. TUCKERMAN: I am curious concerning the distinctions you would make by which these instincts are not inherited protoplasmic memory.

DR. CONKLIN: I should say that they are. I think that instincts are probably inherited protoplasmic memory.

DR. TUCKERMAN: I thought you had made a distinction between instinct and inherited protoplasmic memory.

DR. CONKLIN: No, I think both may be inherited; I do believe that in some way the experiences of individuals must in time be reflected in the inheritance. For example, the things that are brought about by training are similar to those that are brought about by inheritance. The two things are so similar that one very often has great difficulty in deciding whether a character is inherited or not. If we could find a method by which the adaptions of the individual could become adaptations of the race, we should be very greatly aided in the study of the causes of evolution.

Take the adaptations of an individual to experiences that it has never had before. There are multitudes of these. For example, just take the case of immunity to certain bacterial poisons or to certain diseases. Here is something that in certain cases is individually acquired, as with diphtheria or smallpox or measles. If we have had vaccination for smallpox we have acquired an immunity that lasts for a certain length of time. If we have had smallpox usually the immunity lasts for life. The same is usually true with measles. There are cases of inherited immunity in animals and in human beings. There are human beings who are naturally immune to certain diseases that others are very liable to. This natural immunity resembles very greatly the acquired immunity and the question is whether they may not in some way be related, whether, for example, the natural immunity may not be ultimately one that has through long generations been acquired. I think it could be explained, as all adaptations whatever were explained by Darwin, who said that all fitness is the result of the elimination of the unfit. As you know, that is the principle of natural selection. The elimination of the unfit will ultimately leave only the fit, and consequently you have an adaptation brought about by elimination of the unfit.

Now, there is no doubt that a great deal of that is going on in nature all the time. Can we extend that principle of the elimination of the unfit to the acquiring of an immunity or tolerance, the tolerance for poisons, for example? I know of a case where experiments were made in treating protozoa, naked protoplasm, with one of the most violent of all poisons, corrosive sublimate. If you begin with a minimal dose and gradually increase it, you can have these protozoa living in water that contains a concentration of corrosive sublimate that would instantly kill an animal that was put into it without this acclimatization. The same sort of thing is possible with high temperatures; animals can be acclimatized to high temperatures that they couldn't endure at all if they were plunged at once into them.

Take the case of the paramecium that I spoke of yesterday*, avoiding extremes of heat and cold; what was happening during all that process? It was eliminating unsatisfactory responses, that is, it was continually eliminating responses that took it into hot or cold water, and so it stayed in water of moderate temperature.

We adapt ourselves to the conditions that we are placed in by eliminating the useless responses and persisting in the useful ones. Every child does that. Every baby learning to move, learning that it can't reach the moon or can't reach certain things, learning distances, is eliminating useless responses and persisting in useful ones. Now here is the principle of Darwinism, or the elimination of the unfit, extended to individual responses. If we could in some way get the results of such experiences into the germ plasm and have them reappear in the race so that each individual does not have to go through all these experiences, we should have a clue to the method by which the evolution of the race is taking place. But I am hardly ready to believe that this can be done, because all the experiments that have been carried on in animal and plant breeding seem to indicate that acquired traits are not inherited,** and therefore the doctrine of Lamarckism, or the inheritance of individually acquired traits, is no longer a popular one among biologists.

DR. YOUNGBLOOD: Where do we get all this complexity that we call ourselves?

DR. CONKLIN: Well, that is a very important question and one that I think no one yet can answer successfully. If you ask the student of heredity like T. H. Morgan, for example, whose studies on the fruit fly, or Drosophila, are well known, he would say that all kinds of mutations are taking place; most of them are useless or injurious; animals appear with short wings or no wings, small eyes or no eyes. These injurious mutations are eliminated very quickly. Such flies don't leave enough offspring or they cannot compete with normal flies. Consequently, they are quickly eliminated, but occasionally, only very rarely, a mutation occurs which is really beneficial, and that fly has an advantage compared with the normal stock from which it came. His conclusion, therefore, is that evolution has progressed by this slow method of picking out occasional rather rare mutations that are advantageous, and so building up bit by bit the enormous complexity that we find in our bodies and in the bodies of all organisms.

This seems to me incredible, but there are so many incredible things in the world that I don't hold that to be an argument against such a tenet. There are so many incredible things that it might still be true even though it is incredible, and we must remember that time is long; evolution has been going on for an enormous period of time and many things could have happened in a billion years. But still I have difficulty in believing. I talked some about that yesterday.+

* Page 52. ** The apparent exception in the case of MacDougall's experiments (p. 64) is possibly explained by his method of selecting the rats of of each generation for continuing the experiment. Ed.

+ Section II of the lecture, pp. 51 ff.

For instance, consider the evolution of the eye. Darwin said he never thought of attempting to explain the origin of the eye without a shudder, and I can sympathize with him. Think of the transparent tissues in the eye; transparent tissues aren't common, you know. Think of the cornea, the lens, the humors; think of the retina with all its complexities, the intricate musculature for adjusting to near and far objects; the muscles and nerves for moving the eye ball, and you are not through yet. Think of the parts surrounding the eye, the lachrymal glands and ducts, the eyelids with their glands, the hairs on the edge of the eyelids, the eyebrows, the bony orbit around the eye to protect it. My Lord, just think of it! Who is going to explain all this by the process of the elimination of the unfit? I think there is still left a great field for research with regard to the causes of evolution. There are many causes, no doubt, but I do believe that someone, and someone possibly within this generation, is going to bring forth some big principle that we have been neglecting, something that will be comparable in importance to the elimination of the unfit.

DR. YOUNGBLOOD: The carniverous animals have teeth fitted to their purposes, we have our hands for grasping things.

DR. CONKLIN: Everything about us is made as if it were for a purpose. There is purposefulness everywhere in the living world. I didn't have time to talk about that yesterday. It probably makes no difference to a molecule of water whether it exists or not, but for one reason or another it makes a big difference to a molecule of protoplasm whether it continues to exist or not. Wilhelm Roux, who was the founder of Entwicklungsmechanik, said it took so long to actually bring protoplasm imto existence that if once it were wiped out there would be no possibility of its being reestablished except through that enormously long period of building up again from colloids, which we believe was the method in which protoplasm arose. Consequently, because it has taken such an enormous period to bring protoplasm into existence, it is a very precious thing and must be preserved at all hazards. In the living world there appears to be an end in view, namely, the end of survival, which you don't find in the nonliving world. The fundamental value, ethics if you please, of life, is survival. The worst possible thing that can happen to any individual or race, from the biological point of view, is extinction, and therefore everything is set to avoid extinction.

MR. SETTE: You mentioned yesterday that since religion had supplied force and sanction to ethics, and that today we find that religion no longer gives the sanction to ethics that it has in the past, it appears that the only sanction that would hold is that of understanding. Unfortunately understanding brings a rather hard

discipline which I don't expect will ever reach down into all of us, because it is hard to think. Do you have any idea what science will do to replace the moral sanctions or religious sanctions back of ethics that will have the same force with all of us that the religious sanction used to have?

DR. CONKLIN: Well, that is a hard question. The fact is that we must probably always count on there being a large element in human society that will be influenced more by emotion than by reason, and a great deal more by religious emotions than by scientific processes. The wonderful thing about the world religions, and particularly the religion which the Western world has followed for the past two thousand years, is its adaptability to all kinds of people. Children and people who remain childish throughout life read the parables as actual happenings. To more intellectual people these become parables, stories that illustrate deeper meanings. The same thing is true of the words that we use. For example, take the word "spirit." I have often thought if we could only have an understanding of what we mean by "spirit," we should agree a great deal better. It may mean <u>spiritus frumenti</u>, and it may mean almost anything. Its context will tell to many people what it means, but to the average person it is something that is more or less immaterial. If you talk about the spirit of a man, the child or the savage or the person who is not trained will think of a sort of foggy something or other that has the form of a man, a kind of gaseous vertebrate. That is the spirit or ghost, but that isn't what you and I think about spirit. We have a totally different idea about it and it is one of the remarkable things about religion that it does fit the child and the savage as well as the philosopher. I think this will be true in the future as it has been in the past.

Science, however, is going to have more and more influence in bringing people to a rational point of view and in supplementing the idea of a reward in heaven and punishment in hell with more rational, and I think, more ethical sanctions. Milton wrote:

> "The mind is its own place and in itself
> Can make a heaven of hell, a hell of heaven."

Now there you see you get to a scientific point of view and not merely the idea of a place where you roast the poor devils over the fire. I once heard an old negro preacher in Mississippi describe to his audience the sinner who is "hair-hung and breeze-swung over the bottomless pit of brimstone." I tell you it made a terrific impression. Whether it made his hearers more ethical I cannot say; maybe it did.

DR. YOUNGBLOOD: Is it a quality of intellect or an inferiority by which we are able to create these non-existent things simply for our own satisfaction?

DR. CONKLIN: No, I don't think it is an inferiority. I think that we have emerged to a certain higher level in which we see in hell and heaven symbols of mental and spiritual states; and the same thing is true to a large extent with regard to ideas of a future life. I wish I could feel as confident about a material and personal future life after this as so many people do. But the scientific evidence does not seem to me to warrant this, and consequently I take refuge in a justifiable faith in a better world here, looking forward to the City of God on earth and not in the skies. I really believe this is a more ethical idea than the selfish one that some people hold of having wings and a harp and of singing glory forevermore in the heavenly Jerusalem. That, you know, is about on the same level as the Mohammedan Paradise, where you are ministered to by the houris, and all have a grand and glorious time. The common idea of a future state is one of sensuous pleasure or pain, and there is no doubt that such beliefs are losing force as effective sanctions for right living.

DR. DEMING: I understood you to say yesterday that the more we know about nature the more mysterious it seems. Do you mean that it is impossible to say that nature is orderly?

DR. CONKLIN: No, it is orderly, but why is it orderly? How did it get to be orderly? You see we get a bigger and bigger mystery back at the beginning. Why is it that things develop as they do? Development is merely bringing out what is potential in the germ. You can't bring out more than what is potentially there, and yet how did this potentiality get there? Many years ago, John Tyndall, a great physicist and a great mountain climber as well as a prose poet, made one of the earliest ascents of the Matterhorn. And as he sat up there on that crag, he wondered if there was in the rock on which he was sitting the elements of the thought and the feeling of sadness that he then had. He concluded that it must be so, that the promise and potency of all life must have been in the fire mist from which the solar system and the earth and all that inhabit the earth have come. The possibilities of all that has come out of evolution must have been there at the beginning, not actually as realities but as potentialities or possibilities. How did these possibilities get in at the beginning?

DR. DEMING: But is there any use to speculate on something that you can't prove?

DR. CONKLIN: No, but you asked about mysteries, and I said the more we know of nature, the greater are its mysteries. The average mind thinks of every happening as an individual thing, and as such it is explained, and usually rather easily. But when you try to explain the whole system and order of nature, then it does get more mysterious. That is what I had in mind.

DR. SCHWARZ: Dr. Conklin, yesterday afternoon you explained, as I got it, that human conduct and human behavior had their roots in the fundamental properties of protoplasm. Can we, on the basis of some similar agrument, justify democracy on a biological basis? Does democracy have its roots in animals, and aggregations in animal associations?

DR. CONKLIN: I think it does. I gave a lecture last spring in Philadelphia on the biological basis of democracy, and it was published in a series called the Barnwell Addresses.* In that address I first of all undertook to define democracy. That word is like the word "spirit," it has a good many different meanings, and I took as my starting point the charter of our democracy, which is the Declaration of Independence. "We hold these truths to be self-evident, that all men are created equal in that--" I am adding the word "in" but that is, I think, precisely what Jefferson meant, as you can see by the context,"...that all men are created equal in that they are endowed by their Creator with certain inalienable rights." They are equal in these inalienable rights to which they are born, and "among these rights are life, liberty, and the pursuit of happiness."

Every person is born with the right to live. He may sacrifice that right and society may take it from him because he has violated certain codes. He has the right of liberty, he is born with that right but he may lose it by misbehavior, and likewise with the pursuit of happiness. Or, you can take the motto of France, "Liberty, Equality, Fraternity" as the fundamental principles of democracy. If you consider these rights from the biological point of view, you will find that each of them is limited. There is nowhere unlimited liberty in a society nor absolute equality and fraternity, although they are relative and they are also fundamental.

Now, so far as liberty itself is concerned, it is absolutely essential to the dignity of the individual. You can't get the best out of the individual if he is merely a cog in a machine. Biologically it can be proved that liberty within certain limits is essential to the best and highest development of the individual. That I take to be one of the aims of democracy, namely the highest possible development of individuals.

*Edwin G. Conklin, "The biological basis of democracy." The Barnwell Bulletin (Central High School of Philadelphia) vol. 15. No. 62, June 1938.

A great deal has been said with regard to how democratic equality is contradicted by modern doctrines of biology. Of course, we know that men are not equal in their capacity, in their intelligence, nor even in their opportunities. There is nothing more evident than that every individual is different from every other one; no two human individuals are alike except possibly identical twins, and even they show certain differences. We are all different and we are all unequal in that respect; but we are created equal in this other respect, in our right to life, liberty, and the pursuit of happiness. And any society that denies these things to its members is limiting them unduly and indeed is bringing about its own ultimate destruction.

Human fraternity is a fundamental doctrine of biology. All men belong to the same species, Homo sapiens; all are blood relatives, and all are cousins if not brothers. We whose ancestors came from western Europe are not more distantly related than about thirty-second cousins, but although we have common ancestors we may not have the same chromosomes, for in every child one-half of the parental chromosomes are thrown away. We may, for example, be descended from Charlemagne as someone has shown that some of our financiers are. Well most of us, I suppose, are descended from Charlemagne. But not many of us have any of Charlemagne's chromosomes. They have been eliminated or have passed to collateral lines. Many a man bears the name of some distinguished person and is very proud of it when he doesn't have a single one of the great man's chromosomes nor a single one of his distinctive traits.

Well, you see, democracy insists upon measuring people by their individual worth and not by the worth of a family or a name or a class that they may belong to. Every tub stands on its own bottom in a democracy and this is the only natural or biological classification of men, as distinguished from the artificial classification based on social position rather than on individual merit. The democratic classification of men is biological and it is one that gives hope and encouragement to the youth of a land.

Finally, the right to the pursuit of happiness is one that especially affects the people of this age. Happiness is individual; what is happiness to one is not to another. The people who do the manual labor around this Department or on the farms, or anywhere else would not usually enjoy your work in laboratories or labraries. They wouldn't be happy at it and you wouldn't be happy at their work. They want the opportunity of being happy in their own way in their own jobs, again an individual thing. And the one thing in which we can all be most happy is in feeling that we are getting on, that we

are developing. This is one reason why we look back upon our youth as the happy period of life. Most people do so because they were then really making progress. If you can have the feeling that you are making progress, you are certainly going to be happy. And everyone should be able to have that feeling. People should be equal in their right to the pursuit of that happiness.

DR. SEEGER: You were talking last night about the difficulty of solving the practical problems of ethics. Would it be possible for us to use the scientific method in order to find the answers to our ethical problems?

DR. CONKLIN: I think it would. Indeed I am such a believer in the scientific method that I think it is the only way to solve our problems. How else can we solve any problem except by finding and controlling causes? The universe is intelligible, which means that it is causal, and we continually see that the way to control phenomena is to control their causes. We don't know the cause of cancer and we don't know its cure, but there isn't a man who has studied that subject who doesn't believe that the control of cancer lies in the control of its causes. Ethical problems are amenable to scientific methods no less than pathological ones.

MR. BEAN: Dr. Conklin, so much of ethics seems to spring from one's notions of the individual relation to God, the individual relation to a hereafter. You have said that science cannot prove these assumptions with respect to God and the hereafter on the ground that science has to deal with material things that must be verifiable, and here we are relating ethical notions to a field that is not material and therefore cannot be open to material approaches. As we view the world, its past and its probable future for the next thousand years, most of the ethical notions that we find here in the Western world or anywhere else, spring from, have sprung, and will continue to spring from these immaterial sources or our notions concerning the immaterial aspects of life; and if science must admit that it can't get into these fields of immateriality because it has no devices, no techniques, then how can science be of any help to the processes of ethics that spring from those sources?

DR. CONKLIN: Why do you say that science cannot deal with these things? Can't science deal with the mind? Isn't there such a thing as a science of the mind? Science can deal with anything that is. I said yesterday,* quoting Professor Brooks of Johns Hopkins, although I didn't use his name, "Nature is everything that is, "and

*Page 50.

science deals with nature. Science therefore deals with everything that is, the immaterial as well as the material.

DR. CHAPLINE: Dr. Conklin, can I break in just a moment? If I understood your statement correctly last night, you raised some question about the supernatural, and indicated that if we can get out from our mind the personality of deity and look at the qualities of deity, we shall get away from the supernatural. After the lecture I heard one chap say as he went down the hall that he thought he would ask you a question; would you explain the difference between supernatural and the natural, or is there a supernatural?

DR. CONKLIN: I think there would be great gain for both science and religion if we were to recognize the spirituality of the natural, and the naturalness of the spiritual. Personally, I don't believe that there is a supernatural. I believe that everything is natural; and with regard to the qualities of God I think that man has put into his idea of God those qualities which he thinks are the most excellent. Every race and every man makes his God after his own image. Someone has said that God made man after his image, and now man returns the compliment by making God after his image. We put into our ideas of God those qualities that we count the highest and the best, such as truth, beauty, justice, love. What about hate and strife?

DR. CHAPLINE: Some of us do that too.

DR. CONKLIN: Do we do that now? Of course, we know what the Old Testament has to say with regard to a vengeful God. Do modern people today hold such views? They don't preach them any more.

DR. TUCHERMAN: Christ didn't.

DR. CONKLIN: No, He said, "Ye have heard it hath been said, Thou shalt love thy neighbor and hate thine enemy. But I say unto you, Love your enemies ... that ye may be the children of your Father which is in heaven." You see He was giving a totally different interpretation of deity. With increasing knowledge and better ethical ideals our concepts of deity enlarge and improve. If we were to emphasize the qualities of deity rather than the personality of the deity, science would entirely agree with those two great commandments of Jesus, "Thou shalt love the Lord thy God (truth, justice, mercy, love, etc.) with all thy heart, with all thy strength and with all thy mind, and thy neighbor as thyself." That is the ethics of real science as well as of pure religion.

DR. TUCKERMAN: To come back on the point that seems to me has not been clearly distinguished, you say that we must live by reason and not by emotion.

DR. CONKLIN: Well, if I said that I want to hedge.

DR. TUCKERMAN: Our democracy is based upon the pursuit of happiness. Now the happiness of people depends on the things that they think are worth while. Can science ever prescribe those to a man? A man who had a vengeful God can work out a world in which he looks for happiness just as we can work out a world for a God that is not vengeful.

DR. CONKLIN: That is all quite true.

DR. TUCKERMAN: Where do we learn from science that the emotions we have, the things that we want, that we like to think of in God, are really more worth while than what somebody else thinks is worth while?

DR. CONKLIN: I don't mean to imply that science is everything. Life is more than thinking. It is feeling, it is emotion, it is action; those things are all parts of life. The faults of science are that it has dealt too much with the intellect and too little with emotion. The faults of religion are the reverse of this; it has not ministered in this age to the intellect as it should, and consequently the churches have been losing the intellectual influence they once had. The command was that "Thou shalt love the Lord thy God with thy mind," and that has been minimized in many churches; but on the other hand the faults of science are, in general, that it fails to minister to the emotions, and both of these ministries are necessary in life.

Now emotions, as the very word means, are the things from which movement and action proceed. They are the forces that really drive us to do things. The driving emotion of the scientist, for example, is the desire to know; the desire, mind you, which is an emotion. We can't get rid of emotions. We wouldn't want to if we could, but we ought to recognize that some emotions are harmful and others helpful; some social and others antisocial; some ethical and others unethical. People differ in their emotional and ethical ideals. The things that we condemn in the totalitarian States are their wild emotionalism and their primitive and unenlightened ethics. I have faith to believe that time which tries all things, will preserve the fit and eliminate the unfit in ethics as well as in politics. I think it was Matthew Arnold who said "There is a power not

ourselves that makes for righteousness." There is something in the universe, not ourselves, that makes for fitness and righteousness, for rightness, for that is what righteousness means.

DR. DEMING: It seems to me that science does provide or attempt to provide an answer as to just what things are worth while, if I might return to Dr. Tuckerman's question.* If science is the study of everything that is, then it is the business of scientists to find out what things contribute and what things detract from "the more abundant life." It is my own feeling that science is greatly concerned with just this question, and that there is a great deal of energy being devoted to it, and with considerable success. Are not medical research men continually drawing public attention to their pronouncements of scientific reasons why this or that habit of living is or is not harmful? How about Raymond Pearl's recent work on the longevity of smokers and non-smokers? And I have been assured that there is a solid scientific basis for every one of the ten commandments.

Perhaps when science cannot hope to provide an answer to a question, the answer must be after all merely a matter of taste. Some people prefer black tea, others green tea, but such things don't really matter.

DR. CONKLIN: I agree entirely; science is undoubtedly separating the true from the false, the beneficial from the harmful, the good from the bad. Therefore, it does establish values. And it is reassuring to find that the ethics of science does not differ in essentials from the ethics of the most enlightened religions.

DR. DEMING: Dr. Conklin, you said a littlewhile ago** that one fault of religion has been its failure to minister to the intellectual. We understand, of course, that no one can make statements in such topics with hundred percent validity; we all know, I dare say, people of lofty intellect who attend divine worship regularly, no sect being excepted. Yet what you say remains true, by and large, and it is unfortunate, because religion is the way of life, and its chief aim should be and usually is the accomplishment of the greatest happiness on earth for all people. Would you be more specific and say just how you think the churches could draw the support of more "intellectual" people? And another question, don't you think that possibly some of the trouble is with the intellectuals? Are they able to comprehend the objectives of religion as much as we give them credit for?

*Page 74. **Page 74

DR. CONKLIN: What I had in mind was this: There was a time not so long ago when the churches were the intellectual leaders as well as the spiritual guides of the people. Most of our older colleges and universities were founded by various denominations. For several reasons, chief among which I think is fear of and hostility to science and scientific methods, the churches have lost this leadership, which has passed to the universities. It was unfortunate that many churches opposed the findings of science with regard to the age of the earth, the origin of species, the evolution of man, etc., because of supposed theological difficulties.

When scientific theories are erroneous they must be corrected by more exact scientific study and not by theological denunciation. More and more religious leaders are coming to recognize the truth of this, and at the same time to realize that the true function of religion is to emphasize the spiritual values of human life, to cultivate faith, hope, and love; to inspire noble ideals; to be schools of generous ethics. These needs are as great as, or perhaps greater than ever before. They are needs which all serious minded men recognize. They are primal and eternal needs and in all ages they have been met, and must continue to be met, by pure religion.

How could the churches draw the support of more "intellectual people"? I think there is no better way than to emphasize real Christianity rather than Churchianity, ethics rather than theology, religion rather than science. It is perhaps presumptuous in me to attempt to answer this question, but I think that scientists and "intellectuals" as well as the common people would crowd to hear Jesus today as they did nineteen centuries ago. He did not talk science nor creeds nor dogmas, but right living, supreme devotion, all-embracing love. Such religion appeals to scientists no less than to the masses; it satisfies the eternal "hunger and thirst of the heart."

Ten Lectures and Discussions on

SCIENCE: Its History, Philosophy, and Relation to Democracy

held at the Department of
Agriculture Graduate School
under the chairmanship of

M. L. Wilson
Under Secretary of Agriculture

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Edited by W. Edwards Deming
Assistant Director of the Graduate School

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Lecture IV

PHILOSOPHY AND SCIENTIFIC METHODS

by

Morris R. Cohen

The Graduate School
The Department of Agriculture
Washington
1939

Lecture IV

PHILOSOPHY AND SCIENTIFIC METHODS

by Morris R. Cohen
Professor of Philosophy, The University of Chicago

In the auditorium at 5 o'clock Wednesday
29th March 1939

I

It may seem presumptuous for one who is not himself a scientist to lecture to you on the scientific method. But I propose only to raise questions for discussion rather than to instruct you. In doing so I shall proceed on the dictum of the famous American philosopher, Josh Billings, that "It is better not to know so much than to know so much that ain't so."

It sounds rather paradoxical to assert that with regard to the nature of the scientific method scientists themselves are not always possessed of clear and sound ideas. Yet on reflection this ought not to seem incredible. Men who have attained old age in good health are not necessarily competent to tell others how to achieve it. Nor is the successful business man always instructive when he comes back to college and very naively explains to the young hopefuls how he achieved his success, generally attributing it to some platitude that he learned from his Greek professor, or from somebody else who didn't know anything about his business. Now men working in a laboratory generally follow certain techniques that are established habits or routines in their field, so that the trained worker cannot readily depart from them, and he thus becomes almost incapable of making certain kinds of mistakes. But when he begins to generalize about the scientific method as such, he is apt to, and generally does, repeat the conventional (and I venture to think erroneous) views of a lawyer named Bacon, or of a public administrator, such as J. S. Mill.

The proof of the fact that men of great scientific achievement do not always clearly grasp the essence of the scientific method outside of their own field of research is shown by the character of their utterances on politics, religion, and all sorts of other issues.* To avoid invidious personal references, let me cite a round robin signed a few years ago by a number of leaders of American science, in cooperation with a number of liberal Protestant theologians and a number

* The reader may call to mind Dr. Moulton's remarks along this same line; cf. p. 8 commencing at line 22, and p. 18 commencing at line 10. Ed.

of business men. It contained a declaration that according to the teaching of science, "God reveals himself through countless ages in the development of the earth as an abode for men, and in the agelong inbreathing of life into its constitutent matter." Now, no one questions the right of anyone engaged in some branch of science to express the theologic beliefs that he shares with the fellow-members of his religious denomination. But to assert that these beliefs are founded by science is to ignore completely the nature of scientific evidence, the determination of which is the essence of the scientific method. If the statement in question were true, there could not be any atheists or agnostics among scientific geologists and biologists. Now one need not refer to the actual inquiry of Professor Leuba, which shows that atheists and agnostics are more frequent among men of science than among the rest of the population. Nor need one even mention such outstanding figures as Huxley and Jacques Loeb. It is sufficient to point to the general recognition by religious as well as non-religious thinkers, that the methods of proof in empirical sciences like geology and biology are not suitable to establish any proposition about God's plans, or even whether life was or was not breathed into matter.

Nothing, it seems, could be plainer than the distinction between the truths of science, which are verifiable by anyone who repeats the experiments or calculation, and the personal opinions of men of science about questions outside the field of their special competence. Why then is the distinction so often ignored, not only by the public and the press but also by distinguished men of science? I venture to suggest that in the public mind the scientist has taken the place formerly held by the priest, as one having access to absolute truth or omniscience. Very few can follow accurate reports of scientific work, and so most people accept popularized and over-simplified accounts of it on faith. Nowadays men working in some particular branch of science are necessarily laymen in other fields, for no one today can cover all the various fields or even keep up with reliable reports as to what is being achieved in them.

It might be supposed that the scientific habit, of drawing no conclusion except on the basis of rigorous and accurate evidence, could be carried over from the domain of one's training into any other region. But actual instances show that this is not always the case in regard to social, political, and religious issues. The discreet silence that it would generally involve goes against the human grain. How can we be silent when everyone about us is confidently certain?

II

If we distinguish as we must between the verifiable truths of science and the fallible opinions of the individual scientist, we may define science as a self-corrective system. A system of theology, for instance, can not admit that it may be mistaken in any part. Its truths once revealed must remain indubitable. If you doubt you get out of the system and are lost in outer darkness. But science invites doubt. It can grow or make progress, not only because it is fragmentary, but also because no one proposition in it is in itself absolutely certain, and so the process of correction can operate when we find more adequate evidence. But note, however, that the doubt and the correction are always in accordance with the canons of the scientific method, so that the latter is the bond of continuity.

One might draw a parallel in this respect between science and constitutional government. A constitutional government is one in which every particular law or institution can be corrected or abolished by constitutional means. This is not possible in an absolute monarchy, or in any form of dictatorship. In the latter you either accept the whole or else have to overthrow it all. Science may thus be viewed as the method by which any particular proposition, which we now believe to be true, may be corrected on the basis of new evidence. Consider, for instance, the great Syntaxis of Ptolemy. Contrary to a popular impression today among those who do not read him, Ptolemy was a great scientist. He had the real essence of the scientific method in him. His observations and those of others were carefully correlated. His reasoning was closely articulated, and his book is still a good example of a scientific treatise. But he proceeded on a hypothesis that has been replaced by a simpler one, since Copernicus reduced the number of epicycles. Moreover, the Copernican astronomy now fits in with a type of physics that enables us to discover a mechanism for the motion of the stars and to show that this mechanism is precisely the same as the mechanism of earthly motion. The new hypothesis thus enables us to enlarge and correct our previous knowledge, but it does not prove that the old one is all wrong. Newton sometimes relied on Ptolemy. If every new discovery simply replaced all previous knowledge, we should have something like an oriental dynastic change where the new regime kills off all the remnants of the old regime which it displaces. The progress of science is not a complete replacing of the old by the new, but a process of continual self-correction.

If we approach the problem of the scientific method from this point of view, we can, I think, prevent some misunderstandings that hinder clear analysis.

III

Some of the misunderstandings of the nature of science arise from the fact that today scientists are not generally trained in history, i. e, in the interpretation of ancient documents or records. The terminology of modern science has changed so rapidly that it is impossible today for a physicist or an astronomer, unless he is a careful historical scholar, to read the Latin writings of the Renaissance or Middle Ages and really get their modern equivalent. Even those physicists who have some knowledge of Latin find Newton's Principia difficult reading, because his terminology as well as his synthetic method is no longer familiar. On the other hand, professional historians generally do not know much about the history of science because they are not familiar with the subject matter. Thus you have the recent spectacle of a presumably great historian who says in effect that there was practically no science before the seventeenth century; that before that time there was a lot of speculation but no observation of facts; that the idea that we ought to build up science from the observation of facts never occurred to anybody before the time of Bacon, Galileo, and Descartes. All mankind was blind and lived in utter scientific darkness, according to this account, until these three men, about the same time, conceived the brilliant idea that instead of speculating they would observe the facts, and so they gave birth to science. Now that, I think, represents a prevalent myth about the history of science. And it is one of the many myths that the future will record as current in this age which thinks of itself as scientific, but is as full of its own supersition and mythology as any other age.

It is not difficult to recognize the existence of science before the seventeenth century. All we have to do is to look at the works of men such as Euclid, Archimedes, Ptolemy, or Hippocrates. There is not in fact a single one of the sciences of which the foundations were not laid in Grecian times. Whether you take mathematics, astronomy, mechanics, hydrostatics, optics, physiology, zoology, or botany, or any other science, you will find that the elements were clearly expounded before the third century A. D.

However, my theme is not the history of science; I understand that Professor Sigerist will lecture to you on that subject and I am sure he will be most informative. But I do wish to call attention to a myth about the scientific method, namely, that it consists in banishing all hypotheses or anticipations of nature and begins with the observation of the facts themselves. Francis Bacon is the hero of this myth. Bacon was not only a courtier but a lawyer trained in the professional Inns of Court. So he conceived the idea that nature could be studied in just the way that law was studied in his day, that is, by cases. According to this view, if you went out into the field and recorded the facts of nature, according to schedules outlined in the Novum Organum, you could build up all the sciences. (Bacon thought he could have done it himself if he were not so

busy with other things.) This notion, which is dignified by the name of induction, has become so fashionable that nothing is so familiar in the intellectual world as the boast that one's procedure is inductive and not deductive.

Now that is a superstition for which, it seems to me, there is no justification except the initial disinclination to think. Reflection shows it to be absurd to suppose that the scientist begins with an empty mind, with a tabula rasa, on which he writes his observation of the facts. It would be interesting if those who talk this way about induction and the experimental method actually tried it. In point of fact we do not know what to look for unless we have some idea to start with. Actually the people who have made discoveries in any of the sciences have not followed the Baconian prescription of avoiding anticipations of nature. On the contrary, they have always been men who were not only familiar with what was already known in their field, but gifted with many anticipatory ideas which they tried to test. It is when a man of genius hits upon a fruitful idea that great discoveries become possible.

IV

In this connection, it is well to be cautious about fashionable doctrines concerning the advantages of the open mind. It is true that too obstinate an attachment to an accepted theory may be a hinderance to progress,* but it ought also to be obvious that in science as elsewhere, no great achievements are possible without courage. And the persistence in a theory, though the facts seem at face value to be contrary, is one of the ways by which men like Copernicus and Einstein have secured great triumphs. To suppose that the sun does not move around the earth, or that relativity might be applied to gravitation and accelerated motion, seemed at first to fly in the face of the facts. But the scientist must be skeptical as to what are the facts. All too frequently they turn out to be accepted prejudices. One might, therefore, define science as the method whereby we ascertain what are the facts. When we know that we don't need anything else.

In support of the empirical myth in the conventional history of science are the assertions that Kepler discovered the laws of planetary motion by looking at Tycho Brahe's tables, and that Galileo discovered the laws of falling bodies by rolling balls down inclined planes. I venture to assert that one who does not already have the geometry of

* See section IX

conics in mind can not possibly see Kepler's laws in Tycho Brahe's or any other astronomical table. In fact, we know that Kepler tried other theories before he tested whether the recorded observations would fit into the theory of ellipses. Similarly, I have no hesitation in maintaining that no one who tries Galileo's experiment on falling bodies, under the conditions that he tried it (without any machinery or even a highly accurate clock), will see his laws vigorously verified. We know that in fact he arrived at the idea of a simple proportionality between the elapsed time and the velocity acquired in it by a process of a priori reasoning (part of which was fallacious), and that he then made his experiments to test it and was satisfied with the rough results, because of his prior conviction that reality or the book of nature was conceived by the Creator in simple mathematical relations.

Along with this it is interesting to note the persistence of the myth that Galileo disproved the Aristotelean theory that heavier bodies fall more rapidly, not by reasoning but by going to the leaning Tower of Pisa and dropping down two objects of unequal weight. Professor Lane Cooper has recently aroused the wrath of the scientific public by questioning the historicity of the incident.* But so far as I know, no one has yet brought forth adequate evidence to eliminate the doubt thus raised by Professor Cooper.

I must, however, pass over this and raise the more logical question: Suppose that Galileo had actually performed the experiment, what would it have proved? Popular philosophers who praise Galileo for resorting to observation and experiment seldom think of repeating the experiment, and it never occurs to them to remark that feathers, snow flakes, rain drops, hail, and pebbles, do not all come down with the same speed. The most obvious fact is that the resistance of the air, as of any other medium, is not the same for all bodies. It is a commonplace of physical science that only in a vacuum would bodies of unequal weight come down with the same velocity, and that the retardation due to the friction of any medium, such as the air, does depend on the mass as well as on the shape of the falling bodies. Now, Aristotle (who did not believe in the existence of a vacuum) was doubtless wrong when he thought that in the air the velocities of falling bodies would be simply proportional to the masses. The functional relation is more complex than that. But Aristotle's error in this respect was generally recognized in antiquity (for instance by his commentator Philopoemen); and Lucretius gave a true and simple statement of what does happen.**

* Lane Cooper, <u>Aristotle, Galileo, and the Tower of Pisa</u>, (Cornell University Press, 1935).

** A convenient source of Lucretius' statement is the book by Lane Cooper cited above; see pp. 49 and 58.

It is sad to find the issue inaccurately represented in such a wonderfully good book as Einstein and Infeld's Evolution of Physics. Myths all too frequently triumph over science.

V

But let us come closer to our main theme--the nature of the scientific method. And here I am glad to avail myself of a very wise remark by Einstein: "If you want to know the essence of scientific method don't listen to what a scientist may tell you. Watch what he does." And I suppose we should qualify it, "Watch what he does when engaged in scientific work, not when he is taking a holiday or is on a picnic, or discoursing on something beyond his competence." If we take the advice and really follow the actual workings of scientific research, we find that one of its first conditions is a willingness to hold all our information as tentative or provisional. That means a willingness, not to question all things at once, for that is impossible, but to question any one proposition by asking, "Is it so?" Of course, to most people that is generally an impossibility, because so many things seem to us too obvious to be questioned. But it also seems obvious that the earth is flat, and that the sun and stars revolve around us. Generally we do not hesitate to condemn anyone as mentally deficient, who does not see what we do. When no issue of great importance to us is involved, we let the poor simp alone. But if he annoys us we may put him in a lunatic asylum, in prison, or we kill him. But the questioning of the obvious is an essential service in science. And this is true both in regard to principles and what appears as facts.

All human disciplines, so far as they have aimed to be scientific, have not only used mathematics, but have till recently looked up to the Euclidean geometry as a model of a scientific system. We can trace that influence even in politics, e.g. in the Declaration of Independence, with its appeal to certain truths as self-evident.

Euclidean geometry begins with certain axioms from which certain propositions are deduced or demonstrated. And what does strict demonstration mean? It means that we can show it to be impossible to accept Euclid's axioms and not accept his theorems. But for over twenty centuries it never occurred to people to question the axiom that from a point outside of a straight line only one parallel can be drawn. In the nineteenth century, however, a Hungarian, Bolyai, and a Russian, Lobatchevsky, and subsequently a German, Riemann, by questioning that very self-evident proposition, opened up new fields in geometry. After the parallel axiom, others began to be doubted, for instance, that the part can never be equal to the whole. Startling as the last seems, it is yet true that the whole theory of transfinites is based on it. In a similar way the axioms of Newtonian mechanics are being questioned today,

and a new physics is being developed in which we no longer accept the Newtonian conception of time and space as absolute, nor the absolute constancy of mass, nor the constant ratio between force and the acceleration of a body. Notice that Newton has not been thrown to the garbage heap as some popular accounts would have us believe: Newtonian physics is still sound, still true, but not sufficiently accurate for certain purposes. What we have achieved by our questioning is to correct Newton's generalizations by showing that they are only special applications of wider laws under limited conditions.

VI

Now, if you follow me, you can see that the same skeptical attitude must be observed in regard to what seems obvious and self-evident facts. In scientific work we must always be prepared to raise the question with regard to any assumed fact--is it so? One of the distinctions between science and common sense is precisely the fact that in science we are not satisfied merely to observe things and believe all we see. We must doubt, examine, and verify. To do this we must invent all sorts of apparatus and means to avoid seeing what doesn't exist and find ways and means of seeing what to the ordinary man is not usually visible. That most people see things or events that do not exist or happen is shown every day in courts of law. Witnesses often swear in good faith that they saw certain things, and when they are cross-examined it is proved conclusively that they were situated at certain points where what they think they saw was not at all visible. We all tend to see what we expect to see and science must help us to overcome that tendency. But science also invents instruments, like the telescope, microscope, and others, to enable us to see things that are not visible to the ordinary eye. In both of those ways, then, science enables us to answer more critically the question, is it so? The foregoing, however, is not sufficient. One of the main causes that make common sense so unreliable and render our political discussion and our so-called social science so full of interminable controversies, is the failure to formulate our questions in definite terms.

Is man good or bad? Are you a pessimist or an optimist? Do you or do you not believe in the possession of private property; in the rights of man? All who answer these and similar questions in the affirmative or negative are agreed to despise those who refuse to answer before determining what precisely these terms mean.

If our procedure is to be scientific, our terms must be definite. And real definition of terms doesn't mean merely finding verbal equivalence. The clearest way of making our terms definite is by the process known as measurement.

What does measurement really accomplish? The answer is that it enables us to avoid such expressions as "this plant is big" (or small). Measurement gives us a number, which is definite; and the number enables us to correlate the length or weight of any particular object with any other object of the same kind. In that way we avoid all the vagueness of common categories, and by substituting definite terms we make issues clear and definite. When we do that, we can generalize and correlate various relations, and so have a system, enabling us to see things not in their isolation, but in their functioning or interaction with other objects.

VII

If we bear in mind the necessity of both actual observation and the correlation of our observations or measurements into a system, we can avoid all the fruitless controversies between induction and deduction. For it isn't true that we can begin with pure facts any more than with pure theory, independent of facts. We always operate with both, and it is doubtful whether in adult intellectual life we ever find one of these factors alone. There is no scientific problem or research except where one asks how certain known facts are to be explained or their fuller nature discovered, e.g. what causes the heart to beat, or cancer to grow in certain tissues. And this means that we not only have our ideal of what would constitute a satisfactory explanation or completion of our knowledge, but also some anticipation (true or false) of the direction of the inquiry or research that will lead us to the desired result.

Doubtless, as was admitted before, obstinate clinging to old ideas hinders the discovery of new truth. But it is well to remember what the great naturalist Charles Darwin said on this point, that the mischief of false theories is slight compared with the mischief of false observations. For a false theory such as that of the phlogiston or the older views of nerve functions still enabled us to correlate a number of facts and thus reveal some corrections, whereas everything built on a false observation has to be eliminated before progress can be resumed. Moreover, it is neither possible nor desirable to clear our minds of all anticipatory ideas or hypotheses. The facts of nature would not of themselves stream in properly ticketed and labeled into such emptied minds. The real cure for obstinate bias is to multiply the number of possible hypotheses or points of view before us, just as the cure for fanaticism is to become familiar with other forms of belief. This is aided by formal logic which enables us to make our assumptions explicit and then to formulate their contraries or alternatives. This procedure is beautifully exemplified in Lobachevsky's Imaginary Geometry.

It is only when we approach a problem with a number of alternative hypotheses in mind that the process of verification has any sense.

VIII

The term "verification" has, indeed, become a kind of fetish to be worshiped, but not critically examined. Very few have actually analyzed its precise meaning. Now it seems to me that verification should be clearly distinguished from confirmation. Any hypothesis which explains things is to that extent confirmed. But every superstition can be confirmed in that way. If, for instance, you believe that thunder is caused by Zeus shaking his rod, every instance of thunder can confirm it. If you believe that everything a man does is due to his subconscious or unconscious ego, then everything that happens can be interpreted that way. But such confirmation is not verification for it does not exclude the possibility that some other theory may explain it just as well or even better.

Let me give you a concrete example of what I have in mind. A woman goes to a psychoanalyst and in talking freely she refers to a friend of hers by her maiden name. The psychoanalyst pounces on that and says, "You don't like her husband?" and she admits that she doesn't.

Freud uses this to prove that our dislikes prevent us from remembering certain things. Well, that is an explanation of the fact, if it is a fact. But some other explanation might be just as good or even better. For instance, on January 1st, January 2d, and some days thereafter, most of us keep on writing the old year. Is that due to the fact that we don't like the new year or have some sort of prejudice against it? The explanation in terms of habit seems better because it explains so many other things without more arbitrary assumptions. Scientific verification consists not in accepting hypothesis because it explains things, but in comparing two different hypotheses and seeing which explanation is better.

Now consider this question of memory. Why do we remember some things and not others? Formerly we used to talk about the faculty of memory, but that did not provide any basis for the distinction. Now the advantage of the hypothesis that memory is physiologically conditioned is that it enables us to deduce possible experiments as to what kind of physical things or sounds are more easily remembered than others, and the results of such experiments either confirm or refute our hypothesis. Confirmation, therefore, is not itself, a distinctive trait of science, because every belief can be confirmed if we wish to hold on to it.

If you are a pious Mohammedan and believe that everything happens according to the will of Allah; then every event will confirm that faith. But by the same procedure non-Mohammedan faiths can also be confirmed.

The difference between verification and confirmation, then, consists in the fact that the former involves some differential or critical test.

This is clearly illustrated by the experiments of Fresnel and Young on the diffraction of light. If the Newtonian corpusular theory were true, certain results should follow, contrary to what would be the case if Huygens' theory of waves were true, and vice versa. And so an experiment that confirms one but contradicts the other shows the first to be preferable. The actual results of the experiment were contrary to what followed on the corpuscular, but agreed with what was to be expected on the wave theory. This made the wave theory the preferable one. The same is true in regard to the difference between the continental theory of Ampere that electricity acts at a distance, and the English theory of Faraday that electricity acts through a medium. No preferences between the two theories could be scientifically established until we had a crucial experiment. This was the discovery of Hertzian waves--which can be reconciled with the Faraday theory, but not with the continental theory. It is only when of two theories, one explains the facts that are in conflict with the other, that we have real verification. What verification does, then, is not to prove a theory, but to show that of those available, one is the most probable.

It is well, however, to keep in mind that one experiment or observation may be inadequate. Thus for years the believers in the Ptolemaic astronomy urged that the Copernican hypothesis could not be true because if it were true, Venus, being an inner planet between the sun and the earth, should show certain phases like the moon. But since it didn't, the Copernican astronomy was held to be demonstrably false. This was in fact a sound argument against the Copernican astronomy, but was shown to be baseless when people began to use the telescope and the phases of Venus became actually visible.

It seems to me that if we take the foregoing view of verification, we can make some sense out of Mill's canons. Consider Mill's formulation of the method of agreement and the method of difference. The first asserts in effect that if you observe instances of any phenomenon all you have to do is to note what is a common circumstance and you have the cause. Now suppose you want to know the cause of cancer. Obviously there are all sorts of common circumstances that are not the cause of cancer. It is only some differential circumstance, that is to say, some circumstance that is present whenever cancer is present and absent whenever cancer is not present, that can be a cause; and so we are driven to the method of difference. But even that might not be sufficient. Things may be accidentally correlated for a long time and yet not be causally connected. To establish a causal connection there must be an intimate relation; there must be some relation of identity of substance or energy, and Mill's canons give us no clue as to how to find it.

Consider, however, an actual situation when we are approaching a problem with a number of possible alternative hypotheses as to what e.g. might be the cause of cancer. Then Mill's canons formulate with some degree of appropriateness how by the process of verification I eliminate circumstances that fail to meet the requisites of a cause, i.e. of being necessary and sufficient conditions for the phenomenon observed.

IX

Most people draw a sharp distinction between natural science and social science. There is some justification for this, but it is unfortunately misleading as a dichotomy. If there are facts about human nature they are just as much the object of natural science as any other facts. The real difficulty, however, is that students of social science are interested not only in facts of existence, but in promoting certain more or less partisan purposes. Thus whenever we deal with sociology, or politics, or economics, or any of the so-called applied sciences, we are concerned with two different kinds of questions: (1) how things exist and are connected, and (2) how to promote certain purposes.

The problem of human purpose is a difficult one because it does not seem that all human beings do have in fact the same purposes. And if there actually are ultimate differences of purpose, it may not be possible to come to any agreement. In other words, when people really want different things you can't prove that they don't want what they do want.

Now, Mr. Bertrand Russell has said with regard to philosophers that the reason they do not attain truth is because they don't want to, meaning that they prefer to maintain their points of view rather than see the truth. Well, whether that is true or not about philosophers, it is certainly true in social studies. For in the latter field we are much more anxious to achieve certain results, and our minds are thus closed to the possibility that we may be wrong in our fundamental assumptions.

Take a concrete example: There are in the world today at least three different theories concerning what is the best form of government. Now, all of those theories are being tried experimentally in the different countries. Which works best? There seems to be great difficulty in deciding that objectively. Partisans of each one of these theories point to the country in which the experiment is being tried and say: "There, you see how well it works!" Well, the others are not willing to accept their judgment as to whether communism does work well in Russia, whether fascism works well in Germany or Italy, or whether democracy works well in our own country. These questions cannot be settled so far as I can see by the same canons by which we settle questions of physics. Moreover, the problems of social science are much more complicated because they

involve not only the elements of physics and biology, but additional historical and psychologic consideration. And at that we still have many unsolved problems in physics, e.g. the problem of what will happen if you take an ordinary chair and spin it around: the mathematical equations are exceedingly complex.

It is by no means certain that our methods will keep on growing until we do solve all of our difficulties. It does not even seem highly probable that we shall ever reduce all social phenomena to relations so simple that they can be easily dealt with by a manageable system of equations. Nevertheless, the hope is there and the history of science indicates that many things that seemed unsolvable, sooner or later yielded to the human resolution to solve them. This gives us no assurance, but makes hope possible.

SEMINAR.

following the lecture by Dr. Cohen

Office of the Under Secretary
Thursday morning at 9 o'clock, 23d March 1939

Syllabus

1. Scientific discoveries are not made by accident. Anticipatory ideas are required. Observation is not the first step; we have to know what to look for. Pp. 91 - 93.

2. The only safeguard against upholding some one theory too long is to test out other theories as well and see which is best correlated with the observations, and which leads to new fields of fruitful experimentation. Pp. 94, 96.

3. Science is constantly correcting itself. Scientists are always making trouble--never satisfied. A theory that is held today may be replaced tomorrow by one that gives a wider view. Pp. 94 - 95.

4. Some remarks on the testing of hypotheses; p. 97.

5. Broad interests are necessary for progress on new problems. Wider views stimulate the imagination to see new anologies. Pp. 77, 99, 100 - 101.

6. Are mathematics and physics both sciences? Mathematics is a science in the sense that it is demonstrable knowledge. The physicist uses mathematics, but deals with the actual world to acquire verifiable knowledge. Pp. 101 - 102.

7. The logical laws of identity can not be doubted; they are independent of how the mind works; p. 103.

8. The method of science is not to suppress but to encourage doubt. Science does not give us absolute results, but leaves the way open for corrections, and hence for continuous growth. Such an attitude is very distasteful to most people; human inclination is to rely on authority. The progress of science is by no means assured. P. 105.

9. When people are absolutely certain, no progress is possible; p. 105.

10. The public, as a rule, is not interested in the scientific method or in the evidence with which it is necessary to qualify the validity of any proposition; what people usually want is the romance of the results of science. P. 105.

11. Some remarks on the relative advantages of dictatorships and democracies in various situations; p. 106.

12. Technical terminology is often used to produce a pretension that is not justified. An example is the term "social science" applied to most of the social studies as they are now constituted. They are hampered by emotional attitudes, but real fields of science are possible in the social studies. Pp. 106 - 107.

13. Science is not all of life. We walk although we do not understand all about walking, It is frequently necessary for individuals and the government to act today, before taking time for an exhaustive study of the nature of the act. P. 109.

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Proceedings of the seminar

DR. BLAISDELL: Professor Cohen, speaking for myself, I enjoyed your lecture very much last night, and I suppose it is because you said some things that I agree with. If some of the rest of the people here feel differently, perhaps it will be because they disagree with some of the things you said, and that ought to be a good situation for starting the discussion.

DR. SCHWARTZ: Considering the many important discoveries in science that have been made by accident rather than by design, and that the biological sciences in particular have used the method of direct observation with and without instruments of precision; that many of the fundamental discoveries in biology have resulted from observation, and sometimes by persons without training in science: must we not say, therefore, that the method of biological science is largely inductive rather than deductive?*

DR. COHEN: Well, if I assumed your facts, I suppose I should draw the same conclusion, but I should not grant your assumptions to be facts. In the first place, I should not admit that any great discovery in science has been made by accident. I know that conventional history books say so; but every such account that I have checked up has proved to be erroneous. Let me give you some classic cases. It is said that Roentgen discovered X-rays (or Roentgen rays) by accident--just picked up a photographic plate, and finding a picutre of a key, naturally concluded that a new kind of radiation must have caused it. But in point of fact, Roentgen had previously devoted years of study to the different kinds of radiations, so that the accident was the kind that could happen only to one who had thought as much about the subject as he had. It is only because he was ready to see the importance of that particular picture that he discovered the rays that caused it.

Some books also say that it was by accident that Hertz discovered electric waves. But if you read what Hertz himself says you learn that he was for years concerned with the question of whether the existence of electric waves could be shown. He had been set that problem by

*Cf. the lecture, sections III and IV.

Helmholtz to test Faraday's theory of the ether. Otherwise he would not have recognized what he saw as electric waves. Nor do I know of a single important discovery made in scientific biology by anyone to whom it was not a matter of special interest and study. Of course, you can mention early observers like Leeuwenhoeck, who was only a janitor. But I think we must get rid of the notion that a man's intellectual equipment is determined by his social position or office. Leeuwenhoek was, undoubtedly, a keenly interested and close observer, because he was looking for definite things. The ordinary man can look through a miscroscope but he doesn't see anything of importance. I know for I looked through microscopes when I was a student, and could not see anything except patches of what looked like mud. I was told to draw what I saw, but I couldn't draw anything definite when I saw nothing definite. Some ideas are required before you can have really intelligent observation. We see with our mind's eye as well as with our physical eye: and seeing with our mind's eye is precisely what is meant by having anticipatory ideas.

DR. TUCKERMAN: I am wondering whether that is quite clear. I am puzzled about your viewpoint. Take Leeuwenhoek, for instance, when he started out he hadn't a darned idea except ...

DR. COHEN: Well, yes, that is exactly why I questioned ...

DR. TUCKERMAN: Wait a moment! He saw things and then he saw more things, and after a while, he said, "What does that mean?" I have just been reading Dobell's book <u>Antony van Leeuwenhoek and his "Little Animals"</u> (Harcourt Brace, 1932). I could find no preconceived ideas in the excerpts from Leeuwenhoek's writings. The generalizations he made were built out of the things he saw, after he had seen them. He was interested, yes, he had a mind, yes; but did he first have a lot of preconceived notions that he tried to verify?"

DR. COHEN: Most decidedly, yes. My point is this: unless he had anticipatory ideas, he would not have noticed the things that he saw. Take people from the street, give them a microscope with the sort of things Leeuwenhoek had before him and ask them what they see. Try it.

DR. TUCKERMAN: I tried it myself as a kid when I got a microscope, and I saw things I never heard of.

DR. COHEN: If you had seen what Leeuwenhoek saw you would be in his class. (Laughter)

MR. WILCOX: I should like to ask how your theory differs from the more usual type of explanation of the scientific method. To make it concrete, would you indicate the steps in your outline in contrast with the four steps in the classic illustration of the discovery of Neptune, taking as the conventional outline: (1) <u>Observations</u> of the positions of the planets by Tycho Brahe, (2) <u>Induction</u> into the three laws of Kepler; further induction into one law by Newton, (3) <u>Deduction</u>, in the form of mathematical

computations by Le Verrier and Adams, leading to the theoretical position of an unknown planet whose existence was set up as a hypothesis to account for the irregularities in the orbit of the outermost known planet, (4) Verification, when Galle set his telescope in the indicated position and discovered Neptune (September 1846). The point I am asking is how does your explanation of the scientific method differ from those classic four steps?

DR. COHEN: It differs because I do not believe that mere observation is the first step. For, as pointed out before, in order that the observation should be significant, in order to find anything of importance in science, we have to know what to look for. Take the example given in some books--the example that you gave--namely, Kepler looking at Tycho Brahe's tables and seeing the planetary laws.* That is a most absurd story and no one who has ever seen those tables or read any of Kepler's works can for a moment believe it. The fact that the thing is current in textbooks shows to what depth of ignorance reputable writers on logic and the history of science can descend. If ellipses were so obvious in looking at Tycho Brahe's tables, why did not Tycho Brahe himself see them. Why did he invent his compromise between Ptolemy and Copernicus? In fact the figures of Tycho Brahe's tables do not fit into ellipses with perfect accuracy. Kepler himself tells us of his various other attempts to explain the planetary motions before he hit upon the theory of ellipses. Assuming with Galileo that the planetary motions must conform to some Greek geometric pattern he tried the theory that the paths of the planets would be found in the circumscribed spheres of the five regular solids. If you read the introduction to his treatise on Mars, you will see he did not begin with the theory of ellipses, and that it was suggested to him by Apollonius of Perga's treatise on conic sections after other theories failed to fit the observations.

MR. BEAN: You mean in a simple case, where we have two columns of figures, it would be possible to see a curve in those two columns?

DR. COHEN: I should say no one curve is definitely determined by any column of observations. What we actually do is precisely what Kepler did in essence. We try a number of theoretical curves and see which fits best with the actual readings. Anyone who has ever done any curve fitting knows that if you take any column of figures and try to fit a curve to them, you have to do a lot of stretching or contracting of the figures as well as of the curves to make them fit. (Laughter)

I see that this strikes a responsive chord. And, on reflection, there is no reason why it should not. For, after all, nature is not concerned about our analytical difficulties. Nature doesn't run in order to enable us to get beautiful curves. As a matter of fact, the fitting of a geometric curve to actual observations is a problem that can be solved only approximately, and often only by a considerable degree of generosity. (Laughter)

* Cf. the lecture, bottom of p. 81.

SECRETARY WALLACE: Professor Cohen, assuming that there are a million orders of truth ...

DR. COHEN: I don't know what that means. I know only one kind of truth.

SECRETARY WALLACE: Let us go back to the pre-Copernican theories of astronomy. Those theories seemed to explain after a fashion the appropriate observations of that time.

DR. COHEN: That is right.

SECRETARY WALLACE: And then they gradually put on successive orders advancing by theory and experiment, by the procedure that you have described.

DR. COHEN: That is right.

SECRETARY WALLACE: Advancing with that procedure, there is, of course, always the danger of directing the insight of the mind over the facts; there is always the danger that that particular order of truth, which may be a primitive order of truth, will be embraced over too long a period, especially if it is reinforced by colleges or by the church or government or other institutions.

DR. COHEN: Certainly. That is perfectly true.

SECRETARY WALLACE: I see, therefore, a certain amount of danger in your concept; although in some respects it runs entirely contrary to the Nazi doctrine of truth, yet I can see how your application could easily lend it comfort as well.

DR. COHEN: Well, my answer to that is the one I gave before. The only cure for hardened theory is logic, which enables us to see that the theory we propound is not the only possible one. Logic enables us to raise the question, does the theory satisfactorily explain the facts, or will other theories explain the facts just as well or better? That is the only cure that we have against hardened theory. Without it there is no cure at all, since we all see what we expect to see whether it is there or not--and we assume as facts that which we are too lazy or unable to question. What you call attention to is a very serious danger. But the only way to overcome it, it seems to me, is to be aware that many of our assumed facts are but theories. If you are aware that your assumption as to what are the facts involve a theory, then you ask what other theories are available and whether they may not explain the facts much better.* More important still, in

* See section VIII of Professor Cohen's lecture.

actual scientific research, is to ask whether another theory or point of view would not enable us to open up new fields better than the existent theory. The man who is on the firing line of science is not merely concerned with explaining the facts that exist--he wants to find something.* Now some theories give you a better approach to seeing and finding things than others. For instance, on the theory that electricity acts at a distance there is no reason for looking for electric waves or for an apparatus that will create them, whereas, if you take the theory of Faraday, then you have some sort of clue to electric waves, and along that line you may find some phenomena; in fact, electric waves were discovered.

SECRETARY WALLACE: But that makes necessary a certain number of scientists who are always questioning the complete system which has hitherto been set up on the theory that this whole system, perhaps going back for a thousand years, may be falsely based from the standpoint of concrete truth.

DR. COHEN: That is right.

SECRETARY WALLACE: And who is going to back the scientist like that? He is a sure trouble-maker.

DR. COHEN: He is! and I think the genuine scientist must always expect to be in trouble. Things excellent are not attained without trouble. (Laughter)

DR. TUCKERMAN: Dr. Cohen, I had a hard time trying to follow you in your statement about verification and confirmation.** Look back over the history. I have read Newton (incidentally, in the original) and followed his corpuscular theory of light. I have followed the later developments, and have seen the wave theory come in and partially go out. The wave theory was at that time more stimulating to further research than any other. However, it was not verified any more than Newton's corpuscular theory was verified, for today we know that Newton, with his corpuscles, and his fits, was nearer to our present thought about light than were Huyghens, Young, and Fresnel.+

* As Professor Morris put it, "the test of reliable knowledge is accuracy in prediction." (Part I of Lecture VII).

** See page 86 of the lecture.

\+ From Newton's Opticks, (1704) Book II, Part III, Prop. XII. "DEFINITION. The returns of the disposition of any Ray to be reflected I will call its Fits of Easy Reflection; and those of its disposition to be transmitted, its Fits of Easy Transmission and the space it passes between every return and the next return, the Interval of its Fits."

DR. COHEN: I should not admit that.

DR. TUCKERMAN: Because today light is both corpuscles and waves, or shall we say with Newton, corpuscles and fits.

DR. COHEN: That doesn't mean that Fresnel is wrong, though.

DR. TUCKERMAN: No, but Newton's ideas contained more of our modern thought than Fresnel's. Because Huyghens and Young and Fresnel's way of thinking was at the time a more stimulating way of thinking than Newton's is no reason for saying that it was more verified than his. Similarly I want to mention again action at a distance. We can take all of our field theory, and can convert it back by means of retarded potentials to the old theory of action at a distance. We do not ordinarily do so because in most cases thinking in terms of the field theory is a more fruitful way of thinking; easier, better. There are, however, still some problems in which the old theory of action at a distance gives us a clearer picture and makes our thinking easier.* Where, then, does the distinction between verification and confirmation come in?

DR. COHEN: Well, in the first place I don't agree with your statement concerning the bearing of Fresnel's experiments. I think that Fresnel's experiments did show that Huygens' theory was better than Newton's in the form in which Newton originally propounded it. If you look upon light as consisting of material corpuscles, then the question of diffraction becomes relevant and Newton's theory does not explain the phenomenon that Fresnel's experiments reveal. The experiments of Young and Fresnel do point to a difficulty which Newton's original theory cannot overcome, and therefore as between the wave theory and Newton's corpuscular theory, Newton's theory was definitely refuted. Hence, according to the view of verification that I am propounding, the wave theory was verified in the sense that it could not only explain everything explained by the Newtonian theory but also certain phenomena that the latter could not.

Now, today we are in a different position. Today we don't talk any more about light consisting of material particles. Today we talk of electrons and their fields, and as the electron is both a particle and a wave, it satisfies both the corpuscular and the wave theory. This illustrates the point I made before, that when we verify a theory we do not prove it, but only show it to be better than the available alternative theory.** Now in the state of knowledge of the latter part of the nineteenth century the wave theory with its ether was the best available theory. It explained phenomena of light and electricity better than the corpuscular theory.

* The same issue came up in the Morris seminar, which the reader may wish to consult.

** Cf. the lecture, p. 79.

But with the abandonment of the ether and the new conception of the electron and photon you have elements of both theories, Newton's and Huygens', and an entirely different situation.

DR. TUCKERMAN: Must we give up and say Newton was wrong?

DR. COHEN: Yes, we should say that Newton was wrong. And he was wrong in considering mass as an absolute constant and that if we kept on increasing the force acting on it we could increase the resulting velocity indefinitely. To the extent that Newton assumed such a thing he was definitely wrong. That is why we have non-Newtonian mechanics today; it rests on experimental evidences as well as on the relativity theory.

DR. DEMING: Professor Cohen, your distinction between the concepts that you designate as verification and confirmation appeals to me very much, and I see in it a connection with some ideas that J. Neyman and Egon Pearson have been stressing in their theories of testing hypotheses.* In the first place, they have insisted that there is no use testing a proposed hypothesis in regard to some observed phenomenon (sample) unless some other hypothesis could also be an explanation. If the proposed hypothesis is the only possible explanation of a phenomenon, we shall accept or reject it on grounds other than the immediate phenomenon under consideration. If we reluctantly accept it, we are obliged to go along with it, probable or improbable, test or no test, until we see some new light, and devise a new theory. Now in case we do have two or more explanations (hypotheses), we should choose our criterion of test so that it differentiates as sharply as possible between the proposed hypothesis under test, and the alternatives, one or more in number. We want to guard against rejecting a perfectly good hypothesis, but we also need to guard against accepting one that is not as good as another. In other words, our test should be chosen so as to verify a hypothesis (or of course reject it as not verified). We are not interested in simply confirming it. If a hypothesis is a possible explanation of an observed phenomenon, it is confirmed by the phenomenon; but it is not verified until it is compared critically with other possible hypotheses and found to be the best available. This idea has led Neyman and Pearson into a careful examination of the statistical theory of testing hypotheses.

DR. SEEGER: I was just wondering if perhaps there aren't different uses of the word "theory". I always like to use the word "theory" in the sense of its root, theoria which enables you to say a few things. Now Newton standing at a certain position saw a few things. If we go back today and stand in that same position we see the same view, and Einstein has another view.

DR. COHEN: A wider view.

DR. SEEGER: And if we can get a still higher point that will take

* J. Neyman, Lectures and Conferences on Mathematical Statistics edited by W. Edwards Deming (The Graduate School, 1938).

everything in, then we have the best theory of all.

DR. COHEN: That is it. That is precisely what I am maintaining.

DR. SEEGER: May I ask you an embarrassing question? I am going to rely upon your sense of humor. You said last night a very true thing, that the world is full of myths and superstitions.* I have always thought that probably we never get rid of them. It is like weeds, when you get rid of one kind another kind comes up. You were suggesting that we get rid of the myth that Galileo had propounded the well-publicized experiment on falling bodies, and I agree that from Professor Lane Cooper's work,** and from what I have been able to read through the translation of Galileo's work, that there is no direct evidence of that experiment. I wonder, however, if we are not putting up another myth when we say that the experiment has never been performed.

DR. COHEN: All the experiments of this kind indicate what was pointed out by a man that lived long before Galileo, namely, Lucretius.+ Lucretius pointed out that if you take two things, a stone and a coin and drop them in water, they won't sink at the same rate, and he explained that this is due to the resistance of the medium. Now I take it that air is a medium, and like water will offer a greater proportinate resistance to a lighter body than to a heavier body. I assume, therefore, that it is accepted by all physicists that in the air a lighter body will take a longer time to fall than a heavier body. Einstein in his book++ on the Evolution of Physics refers to the supposed experiment from the Tower of Pisa, and says that it proved that objects come down at the same time. When I last saw him, I said, "Dr. Einstein, how can you say that? You know that the air is a medium, and that a heavier body overcomes the resistance of a medium more than a lighter body." To which he said, "Of course you are right."

DR. SEEGER: Now, to come back, though to the original point about the experiment ...

DR. COHEN: Please remember that I don't say that the experiment from the Tower of Piza didn't take place, but I say that if the experiment did take place it wouldn't have proved what the books say it did, theoretically it should have shown that in the air all other things being equal the heavier body comes down first.

* Cf. the lecture, p. 80. ** Reference on p. 82.

\+ As pointed out on p. 82, a convenient source of Lucretius' statement occurs in Lane Cooper's book.

++ Mentioned on p. 83.

DR. WINTERS: Dr. Cohen, I gathered from your discussion that you recognized the weakness in our scientific procedure due to a lack of historical background and that we may be stronger in technique than in other important factors. Consider the mental attitude of weighing evidence. That brings us back to the preparation for scientific work. Is the historical background more essential or the development of curiosity through training? What can be done toward developing this attitude toward scientific research and so on?

DR. COHEN: I should say curiosity is a function of two factors. One is native endowment. Some people have no curiosity; they are listless intellectually. All sorts of things happen and they are not interested. The capacity to be interested, I think, is native. On the other hand, the element of training is not to be overlooked. I think, that all normal children, and many animals such as dogs show some curiosity. Children show their curiosity by asking all sorts of questions. Education either represses or encourages this inclination. The parents and teachers who do not like to admit their ignorance give some fake answer which confuses the child or gets the latter into the habit of being satisfied with verbalisms. Sometimes things like that happen in the universities. (Laughter)

When I was young I taught elementary mathematics in the high school department of my college, and I had a textbook which pretended to prove that every equation had a root. I knew enough mathematics to be familiar with two attempts of Gauss to prove that proposition. The great Gauss found it difficult. But the author of my textbook blithely tried to prove it to Freshmen in a few lines. Needless to add, it was a phony proof. It is that sort of thing that kills curiosity. It is like giving a stone to a hungry man; it will kill his hunger, if he swallows it.

What you say about techniques is, I think, very important. It is important to realize that scientists are not likely to make many grave mistakes in the ordinary laboratory procedures.* For their techniques are worked out before them, and so long as they go on the beaten path they cannot go far wrong. But when it comes to tackling new problems you need more background. That is why I think it is a great misfortune for students to begin to specialize too early. A young fellow makes up his mind to become a botanist and he doesn't take courses in mathematics or astronomy, theoretical physics, or geology. Well, he is not likely to become a great botanist. He may be able to do routine things well, but if he wants to be creative he ought to have his mind awake. I don't say his mind will necessarily be awakened by the actual course given in physics at his college. He may be better off not to take it. (Laughter) But the principle I am asserting is that if the botanist has the kind of mind that is awake to larger possibilities, he will look at problems in botany from a wider point of view and get the wider perspective that you have indicated,

* Cf. p. 77 of the lecture.

and see new possibilities where other men will not.

Take a concrete example: Einstein worked out the special theory of relativity. Then there was a problem before him: how on earth can it be made to fit in with the effect of acceleration? If you are on a train that is moving uniformly, the principle of relativity can be illustrated. But suppose the engineer puts on the brakes or suppose he speeds up. Well, your coffee spills or things behave differently, and that is an experimental fact. How can you reconcile that with the principle of relativity? The ordinary man of science could not get around such a difficulty. But an Einstein, with the genius for real thought, is not stumped. After all what is a fact? We call that a fact which we take for granted to be involved in our sense experience and which we explain in certain terms; and the question may be raised: can we interpret the supposed facts somewhat differently? That is what Einstein did and arrived at the general theory of relativity. May I give you another example? I took a course once in psychology with Professor C. A. Strong. In this course Strong expounded with great thoroughness his theory of panpsychism. In the last hour of the term he said to us, "Gentlemen, there is one difficulty with my theory which I haven't taken up, and that is that it doesn't fit the facts. (Laughter) A few of us laughed, as you do, but he quietly added:

> After all, what are the facts? The facts of psychology are those data of experience which we interpret in the light of the currently accepted ideas. But since I reject these ideas, the theory which I hold cannot agree with the customary interpretations. Therefore, what I have to do is to take all the data of psychology and show how they do fit into my theory; and that requires a course by itself.

DR. DEMING: Dr. Cohen, yesterday I understood you to make the statement that few scientists know what the nature of science really is and I dare say that is true and I don't feel conpetent to judge, but I want to ask: do you think a man can be a good scientist and not have an intuitive feeling either by training or native ability for the nature of science?

DR. COHEN: If a man hasn't an intuitive insight into problems and where they lead to he cannot do first rate work. But that is different from having a clearly articulated idea of what the whole thing is about.

DR. DEMING: Would you explain?

DR. COHEN: Well, let me say this: we must get back to the principle of routine versus the principle of initiative.* In a great deal of scientific work there is much routine. Now in routine work you don't need to know what is going on outside of your narrow field. If you have the training, if you have the technique, you know

* This is the same distinction that Professor Cohen mentioned on pp. 77 and 99.

almost instinctively what you have to do. No biologist, for instance, will commit the fallacy of Freudian psychologists, of having no control to check his observation. Every biologist "instinctively" knows enough to use controls, in the sense that it has become a habit. But when he comes to a new problem, and the ordinary analogies which have been built up in the course of his routine fail, the old habit or technique is no longer sufficient. One must have a wider view to see new possibilities. Thus it isn't every competent scientist who makes great discoveries, or opens new paths. Wider views stimulate the imagination to see new analogies not seen in narrow fields.

I don't know whether I have made my point clear, but I should like to.

DR. DEMING: Yes, thank you. I should like to ask another question. It may be clear off the subject, but I should like to get your opinion: is mathematics a science and is physics a science? How do they differ? Are they both sciences?

DR. COHEN: Because the term science is one of praise, those who quarrel about the word are usually interested in the prestige that goes with it. But the mathematician deals with a subject that is too well estabilshed and too absorbing in interest to make him care whether you call it science or not. It is the sociologist who, not having the advantage of such a definite subject matter, is so much concerned in defending his claim to be called a scientist. (Laughter)

But I want to answer the question. Let me put it this way: If we are interested in a subject, we can change its name. Let us then drop the work science and speak of the kind of knowledge for which there is evidence, that is to say, views or assertions that are not arbitrary but for which there is some support. Now, if you regard science as the ascertaining of propositions for which there is evidence or proof, no one can deny that mathematics is a science. If for instance, you assume Euclid's axioms, is it or is it not true that four circles can be tangent to each other, or that there are only five regular polyhedrons? There can be no doubt that we can prove that it is impossible, absolutely impossible, for Euclid's axioms to be true and the proposition that there are only five regular polyhedrons to be false. That is the kind of knowledge that we acquire when we study geometry. Now if you have a grudge against mathematics, you can define the word science so as to exclude mathematics. But there is no doubt that mathematics is a science in the sense that it is demonstrable knowledge.

Of course, mathematics differs from physics. For in physics we are not satisfied merely to have the kind of knowledge which informs us that if certain premises are ture, certain conclusions must follow. In physics, we want to know whether certain things are so, whether actual observation will fit in to our axioms or primitive propositions. In other words, the physicist wants to know something about the actual world

and not merely about possibilities. He not only applies or uses mathematics, but checks up the result of his deduction by observation on nature. He thus has verifiable knowledge and that is surely what we ordinarily mean by science.

DR. LAURITZEN: Yes, but you said check up the mathematical equations, and that is just the point I am getting at. The mathematical equations are true if the man hasn't made a mistake--perfectly good based on certain postulates following certain rules; his equations are true whether he observes anything to correspond with them or not.

DR. COHEN: I would not quite put it that way. Equations other than identities are not true by themselves; i.e., equations that refer to existing things and formulate laws of physics or of other natural science are true only if what they assert is as asserted. We must not forget the difference between the point of view of the mathematician and that of the physicist. The former deals with necessary relations between all possible entities. If a man like Einstein begins with an equation like that which expresses the photo-electric law and deduces certain consequences, the process of deduction is pure mathematics. But as physicist he isn't satisfied with that. To the physicist, equations describe states of nature and he wants to know whether the deduced consequences hold in fact. Mathematics alone cannot tell him whether the law of gravitation, the law of conservation of energy, or any other particular law, is true or not. That is a question of fact, and that means, ultimately, whether the observations of nature fit into our mathematical equations.

DR. LAURITZEN: Since one can't tell from the study of mathematics whether these equations are true that brings up the question of whether physics and mathematics are the same thing.

DR. COHEN: They are not the same, obviously; for while mathematics is a part of physics, it is not the whole of physics, and therefore the two cannot be identical.* I would go further and say that if we enlarge, as we must, our concept of mathematics and define it as Benjamin Peirce (1809 - 1880) did as the art of drawing the necessary implication of our assumptions, then every study is scientific to the extent that it is mathematical. But mathematics is surely not the whole of natural science.

DR. ENGLUND: You said that if such and such is true, that is, if our assumptions are true, then what follows must necessarily be true. Isn't that also an assumption?

* At this point the reader may wish to consult the remarks made by Professor Morris, Lecture VII, part III; also the first two pages of the Morris seminar.

DR. COHEN: No, it isn't an assumption in the ordinary sense. There is a difference. The ordinary assumption has an alternative, but the assumption of logic has no genuine alternative. Take, for instance, Euclid's axiom that from a point outside a straight line only one parallel can be drawn. That is an assumption. To be sure, for over two thousand years it was generally regarded as self-evident. But when you examine it logically you see it has two possible alternatives: no parallel lines can be drawn, or more than one can be drawn. To a genuinely logical proposition however, there is no alternative. To the proposition, it will rain tomorrow, there is an alternative. Not so to the proposition: it will either rain or not. The logical laws of identity and contradictions cannot be really doubted. For when you express the doubt you have already assumed the laws of logic to make your statement say what it does and not its contradictory.

DR. RANDS: May that not be just merely a product of mind?

DR. COHEN: That wouldn't make any difference in regard to its meaning or validity.

DR. ENGLUND: Does that mean that there isn't necessarily one?

DR. COHEN: No, the question is not merely one of psychology, of how the mind works, or whether you or I in fact see certain things. The question is a logical one, whether any doubt to a strictly logical proposition can be formulated. A concrete example may make it clear. John Stewart Mill, with the courage of his empiricist confusion said that 2 plus 2 may be 4 for us, but might be 5 on some other planet. That is concretely the kind of issue which you are raising. What is the answer to Mill? It is simply to make the distinction that you made before. If you mean that two drops of water and two drops of alcohol will make four drops of the same size, then you don't need to go to Mars to see that that is dubious.* If you take equal volumes of gases far enough away from their critical points you will find two plus two equals two, because the gases interfuse and the two have the same volume as either one alone at the same temperature and pressure. But if you mean by two plus two what the mathematician means, that is to say, assume that there are any two entities that are distinct, and two other equally distinct entities of any kind, then we can show that it has no meaning to say they might as such be five. Every mathematical proposition exhausts the field of possibility.

DR. ENGLUND: Professor Cohen, yesterday you touched upon the social field in the course of your lecture and I should like to ask you a question bearing on it. I understood you to say that the progress of science in general depends upon the capacity for self-correction.

* The reader may wish to examine what C. I. Lewis says on this very point in his Mind and The World-Order (Scribners, 1929), p. 250.

DR. COHEN: That is right.

DR. ENGLUND: Now, under the totalitarian society there presumably would be in many fields less opportunity for self-correction. Would you care to express yourself then as to the general implication of relative degrees of totalitarianism upon the progress of science?

DR. COHEN: Well, let me put the problem in a most general way. The distinctive trait of all science is precisely that instead of trying to squelch doubt by force, it encourages it as far as possible. The primitive habit is that of authority and reiteration which parents and poor pedagogues illustrate when they are asked embarrassing questions. Communities generally try to suppress doubt concerning accepted ways when just doubt raises fear or irritation. In the absence of the latter, we ignore the man, call him a fool, an idiot, or something of that sort.

If somebody wants to disprove the proposition that in Euclidean geometry the square on the hypotenuse is equal to the sum of the squares of two sides, we don't get much annoyed because we know perfectly well we can prove it to anyone who is willing to listen. But if somebody questions something that we can't prove and about which we profess to be certain, then we get irritated. You can see that everywhere. Human certainty generally varies inversely in proportion to the amount of knowledge. (Laughter) Take the ordinary citizen. Of what things is he most certain? Surely not of the things of which he has most information. Suppose he is interested in the lard market and we ask him the prospects. He will be cautious and give us qualified answers, because he knows something about it. But ask him questions on politics, or religion, and he is one hundred per cent certain, because he knows nothing to the contrary. And when you begin to question him on these matters he gets excited because of the fear that he might not be able to prove what he is not prepared to abandon.

Science as a self-corrective system can abandon any one proposition when there is evidence against it, since it does so by its own method and so maintains its continuity. But the unscientific have no such way and hence must cling to their assertions by reiteration and suppressing any opponent or doubter. The non-scientific attitude, however, breaks down in heterogenious or rapidly changing societies. Imagine, for instance, a child born in an Arabian village. He hears everyone say five times a day, "There is no God but Allah and Mohammed is His prophet." To him that is as much of a fact as electric currents are to us. We all believe there are electric currents and we pity anybody who questions that. But suppose the Arab leaves his village and goes to Europe. He meets people who don't believe in Mohanned. Then he is perplexed. He begins to reflect or analyze--that is what we call the disintegration of ideas.

How can that disquiet which comes from disintegration of ideas be dealt with? Of course, one way is to suppress or even kill all the people who doubt it. It is a great mistake to think we don't get unanimity by killing off those who don't agree with us. I think human history shows that that is a fairly successful method so far as human efforts ever are successful. For instance, classical paganism was actually suppressed, in its outer forms at least, and thus eliminated by force. But that is not the method of science. The method of science takes the bull by the horns and says; "No, we are not going to suppress any doubt. On the contrary, we will try to raise as many questions as we can, because if we question everything we shall very often extend the limits of our knowledge, and within those limits we shall be able to build up something enduring or at least capable of continuous growth." Now the disadvantage of science is that it never gives us absolute results, because being a self-corrective system, you must keep the thing open for the possibility of correction. Such an attitude is very distasteful to most people. Most people like to sleep on a pillow that has no doubt in it. The result, therefore, is that this attitude of scientific procedure is very distasteful, and human beings find it hard to keep it up. The progress of science is thus by no means assured. It goes contrary to our inclination. Human inclination finds it easier to rely on authority.

It is a great mistake to think government rests on force. It rests upon the need of people to be governed. And the Good Book illustrates it in the story of how Saul became the first king of Israel. You remember Saul was looking for his father's asses which had strayed. And when they decided to make him king, he tried to hide among the wagons, but they sought him out and insisted upon making him king. That is a psychologically very true account of the nature of government. People dislike not only anarchy, but also to have to decide for themselves. For that involves responsibility and thought which is always painful. Those of us who have tried it know that.

MR. GLICK: Is that the reason that you were so discouraged about the application of the scientific method to social problems?

DR. COHEN: I am not discouraged. I merely want to look fearlessly at the facts as they actually exist. Science makes progress because it isn't absolutely certain. In those fields in which we are absolutely certain there can be no progress. If you have the complete truth, then to that extent there can be no change for the better. Science means progress because it is constantly correcting itself, and therefore there can always be more correction. This holding yourself in balance and saying, "So far the thing has worked, but we must be prepared to modify it," is very distasteful to people who want to know the final results of science. That is why popular science is largely mythologic. What people demand of popular science is not the evidence that is necessary to qualify the validity of any proposition. People don't care about evidence. What they want is the story or romance of science.

Now anybody who tries to formulate any result of science without the qualifications within which it is true, is of course producing a romance, and it seems to me that men like Eddington and others are trying to persuade people to think that they understand the quantum theory, which no one possibly can who doesn't know enough mathematics. Nevertheless, it is easy to persuade people that they do understand, for it flatters their vanity. They are anxious to believe it, and there is nothing that succeeds so well as giving people what they are anxious to have. Thus you get pseudo-science.

And here we come back to your point that the totalitarian powers have a great advantage over the democratic powers, because the democratic powers ultimately assume that human beings can reason and can weigh evidence for changed doctrines or changed policies. The totalitarian powers assume that the people are either averse to or incapable of thought, and prefer their accustomed ways. Generally speaking, they are thus apt to succeed in the routine of life. Government thus always triumphs over anarchy. But there are periods of stress when the good order breaks down because it is a hindrance to the better. That was clearly demonstrated in the last war between Germany and the Allies. Germany was highly disciplined and the people trusted their commanders implicitly. But when these commanders proved in error, when the final victory in the summer of 1918 did not come, the German people collapsed.

Now in democratic governments there is always an opposition, and the opposition encourages distrust of the government to a certain extent, and people are not so completely surprised that the government can't deliver the goods all the time. Hence defeats did not so readily break the morale of the French and English.

DR. ENGLUND: Would you conclude, then, that on the long pull democracy is more efficient than totalitarianism, although on the short pull you would grant the advantage to the latter?

DR. COHEN: If we last long enough we shall outlast the dictators, yes. (Laughter)

DR. BLAISDELL: Professor Cohen, what you said about sociologists a while back in a way answers the question I am going to put to you, but still I am going to ask it anyway. It is very common these days for the so-called man in the street, and moreover a good many people who are supposed to be intelligent, to refer to a certain body of knowledge as the social sciences. In the first place are we presumptous in using the appellation "social science" to apply to that field of knowledge? And in the second place, does the habitual use of it, and popular use of that phrase encourage generally greater expectation from the people who are studying in that field than we have a right to expect from them?

DR. COHEN: I would answer the second question categorically in the affirmative. I should think that the use of the term social sci-

ence tends to produce a pretension that is not justified and therefore inimical to the very spirit of science. The false pretense or expectation is aroused especially through the use of technical terminology.

One of the great revolutions in the intellectual history of mankind occurred at the end of the seventeenth and the beginning of the eighteenth century, and that was the change from the use of Latin to the vernacular as the language of the learned. Even in Harvard College, I am informed, all the recitations were in Latin up to the Year of Grace 1828. Now, when you speak Latin you are a learned man. You don't have to do anything more. But when you translate the thing into ordinary English then you must say something in order to keep up your prestige as a learned man. That is what happened in some German and Scottish Universities at the end of the seventeenth and at the beginning of the eighteenth century when men like Thomasius, Hatcheson, and Adam Smith began to lecture in the vernacular. Thomasius was almost killed for it by his fellows who saw the danger. But the old professors were needlessly afraid. For by substituting technical terminology for the vernacular they produced the same old situation over again. The ordinary man who wants to know what life is reads Herbert Spencer, and when he is told it is the sum of the vital activities or functions he feels that he has found the secret and he is satisfied. Or when he is told that there are processes of integration or reintegration, he gets the impression that he is in the presence of profound science. Starched terminology gives one the feeling of high intellectual society. In that way, I think, the false appearance of the term science is a real hindrance to progress in actual knowledge. We must get rid of the appearance of saying something when, in point of fact, we are engaged in empty formalities.

Most human conversation is really meaningless, and it isn't supposed to have any meaning provided it conforms to the proper form. It is just ceremonial gesture.

But to get your first question, I should say it is a great mistake to try to rule out all social studies as unscientific because of the false pretense on the part of some who haven't any insight or subject matter and therefore have to quarrel about their terminology. I think there is a great deal of genuine critical knowledge resting upon good evidence in most of the social studies, and I would make a strong plea for history as a science, not in a sense that it sets up laws (that is nonsense, since history deals with individual facts)* but in the precise sense that it deals with evidence critically. Did Louis the 16th conspire with the emigre nobles? The scientific historian examines the available evidence and considers the various possible interpretations and the difficulties they involve. I say that men of that type deserve the name of scientist just as much as geologists or natural-

* There seems to be a disagreement with Dr. Sigerist's point of view; cf. p. 129.

ists of the type of Darwin.

I should then answer your first question by saying that there are real fields of science in the realm of social fact. The man who has an eye for facts, whether it be about human beings, stones, or anything else, the man who asks, "Is it so?" and "Can it be verified?" has the essence of all science, provided he follows the critical methods of proof and verification. The difficulty is not only that social facts are more complicated and thus have a high variability, but that we are under pressure and in a hurry to produce results. But genuine scientific results that will bear criticism and withstand the attrition of time cannot be produced in a hurry. They have to be submitted to all sorts of tests. Hence those who have the genuine scientific spirit always resent being hurried. They tell at Johns Hopkins that President Gilman who was very anxious to get the men on his faculty to publish, went one day to Rowland's laboratory and asked, "Dr. Rowland, have you published anything recently or are you contemplating any publication?" Rowland then said, "Go away, go away. Don't you see I am busy?" Now, I think Rowland, who was one of our greatest physicists had the right point of view. He was engaged in a problem and he was not going to publish things because the university needed credit.

Let me mention something that has a bearing on both your questions. When in the meeting of the Royal Society the result of the expedition to test the Einstein theory was announced, and Newton's law of gravity was thus in a limited sense overthrown, one man is reported to have gone out shaking his head dolefully, "And to think of it, that the Royal Society should listen to a paper attacking Newton who was once its president!" Now, that man seems to me to express the essence of all that science is not. Science was perfectly willing to honor Einstein, who was a German and a Jew, and to overthrow in part the theory of the national hero, Newton. Science is willing to honor the innovator because science doesn't depend upon loyalty to an accepted doctrine, but is willing to overthrow any doctrine for any other for which there is better evidence.

Now if we have that attitude in the social field we can make progress. But what do we have? In the social field we find that certain doctrines are taboo and certain doctrines are hallowed. How many of us do not recoil at the word Bolshevism, Communism or Faschism, free love, atheism, or fail to be thrilled by the words democratic, progressive, etc. But since these words are vague and cover multitudes of different things, free inquiry is hindered by the emotional attitude.

MR. BEAN: I couldn't help but try to apply your suggestion with respect to the desire to get results with accuracy in the very narrow field of administrative economics. A very specific example, let us say, might be where the question arises, what volume of industrial activities is required to give us an eighty-billion-dollar national income? Now some of us working together can answer that question within twenty-four

hours. We place together certain bits of information and we approximate the answer. We feel intuitively that the answer will be within five per cent correct, that we might have to have a fifty per cent increase in production--forty-five per cent to fifty-five per cent, or something of that nature, and for administrative purposes that answer is sufficient. On the other hand, you will find throughout the government circles the attitude of the economists that they mustn't be pressed for an answer today or tomorrow, and that if we set up a dozen people to answer that question perhaps within the next six months we should have the exact answer.

Now, isn't there some danger of the economist and the sociologist trying to be scientific in your sense, while meanwhile the parade will pass by and there will be no need of his answer by the time he gets it, though a ninety per cent answer would have been sufficient if delivered on time?

DR. COHEN: What you say is perfectly true. But we must distinguish between two different questions. One is how to live, and what is the nature of things? Now, unfortunately we have got to live today before we can find out the nature of things. What you are calling attention to is the fact, for instance, that the government has to function today, and that the true result of its action might not be found in six months--indeed, it might never be found. Consider the exact causes of bodily locomotion. What is it really that makes the nerve "conduct" and the muscles contract? To me the question hasn't been completely answered as yet. But that is no reason for my not walking. When you have to go somewhere you walk along and don't bother about the scientific problem of nerves and muscles. Science is not always a pre-condition for other human activities. Science satisfies primarily, I should say, an intellectual interest.*

Now man is not only an animal that consumes food or surrounds himself with various kinds of clothes and buildings, but he also has this curiosity and wants to find out things. Man's various desires sometimes clash. The desire to know is one thing and the desire to make the government function is another thing. Now I do not believe with Plato that government should be run by philosophers or scientists. Government must satisfy other demands than that of human curiosity. Although no government can really function well unless it does appeal to the imagination of its people, it has a number of daily chores, which must be done whether our knowledge is adequate or not. Government cannot stop its functions to wait until we find out all that is involved in a proposed action. That we may never be able to find out.

But that is no excuse for saying that something is science because

* The reader may wish to call to mind Dr. Moulton's similar statement on p. 3 of his lecture.

we do not have time to find out whether it is true. What is inexcusable is not the fact that we have to act on inadequate knowledge, but that philosophers put up sad limitation as the road to or substance of the truth. To say that where we can't get the full truth, we should believe that which we know isn't true, is a perversion or corruption of human intelligence against which genuine science is a protest. "We live forwards and we think backwards," said the Danish philosopher, Kierkegaard. We are born into the world before we think, and it takes some time before we learn to do so, but meanwhile we want to live. So what you say is perfectly true, the government has to live. The streets have to be kept clean. Certain individuals have to be prevented from indulging natural instincts for mischief. All of these things have to be done, and those scientists who think they can solve all problems before we have to act are over-optimistic.

DR. BLAISDELL: You made a statement just now Professor Cohen, which to me at least would open up a whole new field of discussion, when you say that science satisfies primarily an intellectual interest; but since the clock says that it is within two minutes of 10:30, I think we must forego a discussion of that. We appreciate very much your coming, Dr. Cohen.

Ten Lectures and Discussions on

SCIENCE: Its History, Philosophy,

and Relation to Democracy

held at the Department of
Agriculture Graduate School
under the chairmanship of

M. L. Wilson
Under Secretary of Agriculture

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Edited by W. Edwards Deming
Assistant Director of the Graduate School

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Lecture V
THE HISTORY OF SCIENCE AND ITS PLACE IN CONTEMPORARY CIVILIZATION

by

Henry E. Sigerist

Director of the Institute of the History of Medicine,
The Johns Hopkins University

The Graduate School
The Department of Agriculture
Washington
1939

Lecture V

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In the auditorium, 4th April 1939

I

I should like to begin by raising the question of why we study the history of science. Why do we undertake the tremendous labor of digging out historical sources, of interpreting them, and of recreating the past in writing history? I think we do it primarily for two reasons. One is that we should have a very inadequate picture of former civilizations if we omitted science from it. A survey made by Professor Lingelbach of the University of Pennsylvania revealed that the majority of our history textbooks hardly mention science and utterly fail to appreciate the tremendous part played by science in the development of civilization.

On the other hand, we study the history of science, and history at large, in order to understand better the world in which we live. Every situation that we are facing is the result of historical developments, and unless we are aware of them we cannot act intelligently. History is and always will be the great teacher of life. I like to compare the work of the historian to that of the psychiatrist. In examining a patient the psychiatrist endeavors to make unconscious complexes -- often the result of previous forgotten experiences -- conscious, so that the patient may face them openly and overcome them rationally. In the same way the historian, by analyzing developments tries to make unconscious trends that are influencing society conscious so that we may become aware of them and act rationally. The study of history is not a luxury but a necessity because history is a strong determining force in our life.

The history of science has been sadly neglected in the past. This is due, to a certain extent, to the difficulty of the subject. No man can have historical training and at the same time be equally at home in mathematics, physics, chemistry, biology, and other fields of science. Investigations in the history of science often require the cooperation of various experts, and this is why a research insti-

tute for the history of science is badly needed. Such an institute would not only advance research tremendously, but it would be able to make a most valuable contribution to education. It could bring history to the scientist and science to the historian, and could prepare a generation of teachers of history and of science who could approach their subjects from a much broader angle. Everybody agrees that the gap between the humanities and science should be bridged. If the scientist is to be more than a technician and is to play an active part in the life of society, he must by necessity have a broader background, and the teaching of the historian will never be forceful unless he is aware of the part played by science.

Most textbooks of the history of science are unsatisfactory since they are written from a rather narrow point of view. There are still historians who view the history of science as the free play of personalities of genius. The bio-bibliographical approach still dominates in most of our textbooks. Sometimes an attempt is made to trace the history of definite ideas or to picture the general cultural and philosophic background of a scientific period, but hardly ever is an effort made to dig deeper so as to uncover the roots of the phenomena. Nobody will deny the part played in the history of science by men like Galileo, Newton, or Leibnitz. History is made by human beings but men do not live alone. They are members of societies through which they are conditioned. I am convinced that there have been potential men of genius at all times and in every country. It was not divine providence that created them; the economic and social conditions of the society in which they lived determined whether they could develop their genius and in what direction they would apply it.

II

The subject that has been assigned to me is extremely vast, and I therefore have to restrict myself to the discussion of some principles and more particularly to the relation between science and democracy.

There is a connection between the rise of democracy and the growth of science. Both democracy and science are the result of definite economic and social developments. It is impossible to establish a simple causal relationship between democracy and science and to state that democratic society alone can furnish the soil suited for the development of science. It cannot be a mere coincidence, however, that science actually has flourished in democratic periods.

There can be no doubt, for example, that science thrived in the Greek city-democracies where it was cultivated by men who, like Plato, were nevertheless strongly opposed to the democratic form of government, or who, like Aristotle, accepted it very reluctantly. And again science flourished, more than ever before, in the great democratic period that followed the French Revolution, during the nineteenth century. Greek science, however, reached its peak in Alexandria at the court of the Ptolemys. A great scientific development took place in the sixteenth, seventeenth, and eighteenth centuries and was by no means limited to England and Holland. During the nineteenth century science was cultivated just as much in imperial Germany as it was in democratic France or England. And great scientists were found even in the stronghold of autocracy, Tsarist Russia.

The same basic forces led to the rise of democracy and of science. A brief historical analysis will make this evident.

Democracy, although greatly discussed by the Greek philosophers, was not more than an episode in the ancient world. The Athenian state undoubtedly realized many democratic postulates, but it was a democracy of the free citizens only, while the majority of the population consisted of slaves. It broke down in the fourth century and never again was democracy fully effective in the ancient world, not even in republican Rome.

The chief incentive to scientific research was given by the fact that the Greeks were a seafaring nation. Navigation requires astronomical knowledge which in turn calls for mathematics. I know that there was what is commonly called "pure" science in Greece also, but I think that the distinction between "pure" and applied science is obsolete. Practice always requires theory, and theory leads to practical application. Theoretical and practical considerations were more harmoniously combined in the work of the first Greek scientists, the pre-Socratic philosophers.

The centers of commerce became the centers of scientific research, Alexandria, and later Rome, the great cities that imported natural products from all over the ancient world and where growing material demands had to be satisfied.

The fact that ancient economy was slave economy was a handicap to science. So long as labor was easily available and cheap, there was no demand for labor-saving machines. Nor was there any demand for increased industrial production so long as the majority of the population consisted of slaves and poor farmers and only the needs of a small upper class had to be met. The principle of the steam engine was recognized but never applied, and many of the in-

genious machines and apparatus of Hero of Alexandria remained on paper as there was no economic need for them.

In the feudal world of the Middle Ages the form of production and the structure of society were almost static. Man was born into a status which determined his way of living. The tenant farmer delivered part of his crop to his lord in exchange for the protection he received or was supposed to receive, from the terms of a contract. The artisan in the city produced goods according to strict regulations issued by the guilds, powerful organizations whose function it was to transmit a definite body of skills, to preserve standards of quality, and to eliminate competition.

Similar views dominated the institutions of learning. The medical faculty of a university was to a certain extent the physicians' guild. Like the craftsmen's guild it transmitted a body of knowledge, kept traditions pure, regulated conditions of practice and eliminated competition. It is obvious that incentive to scientific research was small in such a static society to which the idea of progress was foreign.

And yet in spite of all, some progress was made in the field of technology. The different conditions of climate and soil north of the Alps called for improved agricultural methods, and the fact that medieval economy was not a slave economy led to a better utulization of animal, water, and wind power.

III

Conditions changed radically toward the end of the Middle Ages when feudalism began to disintegrate. The volume of trade increased and created a great demand for gold as a medium of exchange. Voyages of discovery were started to seek for the much coveted gold. The discovery of the world, of new continents unknown to the Greeks, of new races of mankind, new species of animals and plants, became a great experience and a most powerful stimulus to science.

Problems of navigation were acute once more and became a challenge to scientists. The long-distance voyage required larger and faster ships and improved methods of orientation. The waterways were the chief highways of traffic. The journey from Constantinople to Venice was three times longer by land than by sea, and while the two-wheeled ox-cart could not carry more than two tons of goods, the average-sized ship transported more than six hundred tons. When we study the work of the physicists of the sixteenth and seventeenth centuries, we soon find that practical problems of navigation played a very important part in their studies.

While navigation developed, firearms were increasingly used in warfare and the demand for copper and lead grew. Shallow deposits were exhausted, and it became necessary to dig deeper, which required more elaborate machinery and increased the dangers of mining. It is not by accident that the first monographs on occupational diseases were written in the sixteenth century. The development of artillery raised problems of ballistics which again demanded solution by physicists.

A new economic order began to develop which called for free initiative and free competition and challenged the individual in man. A new relationship between employer and worker was gradually established. Capitalism could not develop in the rigid atmosphere of the Middle Ages. The traditional authorities were fought. The church was opposed and was "reformed." A new political philosophy arose: the philosophy of liberalism. The scientific authorities such as Aristotle, Galen, and Avicenna were violently opposed, and as most universities preserved their medieval character until the eighteenth century, clinging helplessly to antiquated traditions, academies were founded and became the really active centers of scientific research.

For three centuries the battle between feudalism and capitalism went on. On one side stood a landed aristocracy and its parasites, fighting to retain its inherited privileges but losing them one after another. It was aided by the church, which since the early Middle Ages had become one of the great property owners. On the other side was a rising middle class, open-minded, progressive, fighting for political equality, producing great scientists, applying their discoveries in industry and trading commodities on an ever-increasing scale. By the end of the seventeenth century the merchant class had an influential position in the British Parliament, and after 1715, members of the third estate in France were permitted to acquire land of the nobility.

Through the industrial revolution the middle class won its economic, and through the great French Revolution, its political battle against feudalism. In the nineteenth century the bourgeoisie was in power in many countries, and where it did not actually control the state its economic power was nevertheless so great that the state had to follow its lead. The capitalist system of production could now play freely. It required science; and democracy appeared as the ideal form of government so long as it could be controlled by the bourgeoisie.

IV

We can now discuss what the results of these developments have been and what place science occupies in a democratic society like ours.

There can be no doubt that the general standard of living has been raised considerably. Life has become safer and more comfortable. Technology has produced an infinity of most enjoyable gadgets. Science is worshipped. Whoever has to sell a new toothpaste, breakfast cereal, or patent medicine, advertises it as being scientific, and this is considered as the highest recommendation. A man like Einstein is highly esteemed, although most people have not the faintest idea what his contributions have been. He is worshipped nevertheless in an almost mystical way as a symbol of science.

And yet as soon as we take the trouble to look under the surface, we must admit that science actually plays an inadequate role in our society. Our capitalist system of production, the basis of our social life, a system that produces commodities chaotically, in fits, and leads to periodic depressions, is anything but scientific.

Industry applies scientific principles in production but by no means freely or rationally. The chief responsibility of the industrialist is not towards society but towards the stockholders, and a new invention that may benefit society but will not be profitable to the undertaking is suppressed without hesitation.

Does our government act scientifically? Certainly not. Science is not an integral part of government activities. The statesman as a rule knows nothing about science, just as the average scientist is a perfect child in matters of economics and politics. Every government of an industrial commonwealth has to consult scientists, and hundreds of scientists are in the government service as expert advisers. Policies, however, are mostly the result of a compromise. Whenever the government attempts to solve a problem scientifically it comes into conflict with the vested interests of a small but powerful group of the population. And by the time a compromise has been reached the solution is no longer scientific.

Your Department teaches the farmer how to increase the fertility of the soil, and how to produce larger and better crops, but you cannot teach them how to sell their products and you cannot prevent having a million farm families on relief. You have produced a Food and Drug Bill which was scientifically conceived and would have remedied an untenable situation, but immediately you came into conflict with vested interests, and as a result the Bill has been so utterly emasculated that it does not serve the purpose any longer.

Foreign policy is still less scientific. Under capitalism it is impossible to make the sources of raw material available and to open the markets in a rational and scientific way, just as it is impossible to preserve peace under a system that necessarily leads to imperialism. Foreign policy, therefore, is a mixture of bluff, violence, and compromise.

V

Only democracy, we hear, can guarantee freedom of scientific research and can protect science against interference by outside powers. Is science actually free in our society? The scientist in order to make a living has to sell his labor power to some institution or undertaking. In this country there are fortunately many academic institutions in which the scientist has the freedom to select the subject of his researches and to publish and teach what he considers the truth. Suppression of academic freedom is less likely to occur in the natural than in the social sciences. The scientist, however, is not free in many public schools. There are still regions in which the theory of evolution may not be taught and where textbooks must be written in such a way that they will not offend the religious feelings of certain people.

The scientist who works in industrial undertakings naturally must give up much of his freedom and, as we mentioned before, constantly runs the risk of seeing the results of his labor suppressed for economic reasons.

In other words: science today has infinitely more to give than society actually receives.

It seems that the scientists are gradually awakening and becoming aware of the part they could and should play in social affairs. This became evident in the last two meetings of the American Association for the Advancement of Science.

An appeal has been made particularly to the scientifically trained youth of the country to cooperate in the solution of social problems. This is a direct challenge to those of us who as teachers of scientific subjects are responsible for the younger generation. And here we are touching upon a sore spot in our educational system.

Science, from the elementary school to high school and college, is taught in a more or less dogmatic way. The student does not feel himself stirred or challenged. His intellectual

curiosity is not sufficiently aroused and science is not presented to him in its cultural setting. A body of generally accepted knowledge, simplified and carefully digested, is transmitted to the student who accepts it as a matter of course.

Our graduate teaching of science trains specialists, highly efficient specialists who, however, are totally uneducated outside of their narrow field and therefore utterly unprepared to take any part in social action. If the German academic world surrendered so readily to reactionary forces, it was largely due to the fact that it consisted of men who were specialists and nothing else.

If we wish to educate a citizen able to think in terms of science, and a scientist prepared to participate in social action, we must change our methods of teaching. One, and in my opinion the most promising way, is to approach the sciences not only technically but historically, philosophically, and sociologically.

SEMINAR

following the lecture by Dr. Sigerist

Office of the Under Secretary
Wednesday morning at 9 o'clock
5th April 1939

Syllabus

1. How can the study of the history of science be pursued? Pp. 114-123.

2. The circulation of the blood was not discovered until people began to think in quantitative terms; p. 123.

3. Science and democracy spring from the same underlying force, though they are not always found together. Pp. 124; 131-132.

4. We are not profiting by all that science is capable of doing for humanity. Pp. 124-126; 128-129; also section IV of the lecture.

5. Medical care in Russia; pp. 127-128.

6. In a democracy the individual is the supreme end; pp. 127-129.

7. Are there laws of history? P. 131.

Proceedings of the seminar

DR. DEMING: I cannot reflect on this series of lectures and conferences without realizing how disappointing it must be to the Under Secretary that by the irony of fate sickness should again deprive him of the privilege of being present, after he had devoted so much thought and energy in planning the arrangements. He is bearing his misfortune patiently. I might explain too that Dr. Blaisdell also is not here this morning, having been called away for about two weeks.

Dr. Sigerist, this seminar is a conference at which we hope to get different points of view from one another. This is hardly a place for argument, but rather an opportunity to provoke thoughts on the topics that you opened up in your superb lecture last evening. We have a feeling that disagreements, if any occur, might be ironed out in time, but we don't try to convince each other in the space of one conference; rather we try to catch glimpses of each other's views so that we may have something to think about.

You made the important statement last night that in order to get a perspective of any social or political trend it is necessary to investigate the history back of it, and that a study of the history of science of any period is necessary if one hopes to get a clear view of the story of that period. One of our previous speakers also mentioned that scientists are usually not students of the history of science.* For one thing, it is necessary to read in languages that are no longer ordinary--you yourself have found it necessary to learn fourteen different languages--and this brings up the question of how the interests of scientists can be directed toward the history of science. Should a reasonable fraction of the efforts of scientists be so directed; if so, what do you consider the best way to go into the study of the history of science and what is the best way to make it interesting? You made the statement yesterday that the study of the history of science is very difficult. Of course some students who are attending academic institutions have the privilege of attending lectures by eminent scholars (I am thinking of how fortunate your own students are) but such opportunities can be enjoyed only by a few. Even here in Washington where we have access to some of the world's best libraries it is not an easy matter; rare books must be chased down, begged, and borrowed, and compared. It is no wonder that many scientists are not well versed in the history of science. In my own case I have found it desirable to look at some of the developments of mathematicians from the 17th century up to the present since I have become convinced that the development of many topics in their historical approach is the most easily grasped by the mind, either

* Prof. Cohen, Lecture IV, p. 80.

for teaching or one's individual study. Do you think that the same can be said of other scientists and will you tell us how studies in the history of science should be attempted by people who must devote the major portion of their efforts to the prosecution of their project in the present?

DR. SIGERIST: There can be no doubt that the historical approach to problems of science has great educational value. The best way to make a complicated subject clear to students is to trace its development. As a young student, I was a very poor mathematician, and I never made any progress before I began to study the history of mathematics. Then, all of a sudden, complicated subjects became clear. For many years I gave introductory courses to young medical students in which I endeavored to teach them the elements of medicine. I soon found out that the best way to explain to them our present concept of disease was to tell them what possibilities there are of approaching the problem of disease and what different concepts had been evolved in the course of time.

History must be studied from historical sources, and whoever wants to become familiar with the history of science must first of all read historical scientific texts. As it is impossible to master all languages, it is very important to have the chief classical texts of science easily available in modern reprints and translations. In Germany, Ostwald founded a series that contains many hundreds of classical texts. George Sarton has also reprinted a number of such texts in Isis. In the field of medical science the Bulletin of the History of Medicine that I am editing publishes reprints and translations of important texts which can be purchased separately. Eight numbers have already been made available, and every year we publish three or four items. Another series of medical classics with biographical introductions and bibliographies is edited by Dr. Emerson Crosby Kelly (Williams and Wilkins).

In regard to texts, George Sarton's Introduction to the History of Science is the most important such guide, but in three big volumes it has not progressed further than the Middle Ages, and many more years will be required to complete this great undertaking. Most textbooks of the history of science are unsatisfactory because the bio- and bibliographical approach is still dominating in them. Most of them fail to emphasize the unity of civilization. Science is but one aspect of general civilization and reveals the same trends that are also observed in art, philosophy, religion, and all other manifestations of culture. William Harvey, the discoverer of the circulation of the blood, made anatomy an "anatomia animata." Static anatomy became dynamic anatomy or physiology in his hands; and he reflects the same trend that is observed in the work of Galileo who introduced dynamics into physics; or in the work of Michelangelo who in his later

creations initiated a new style in art which is characterized by its dynamic outlook--the art of the baroque.

The textbooks of the history of medicine all follow a pattern that was established in the 18th century, and according to which the history of medicine is pictured as the history of great physicians and their books, or the history of schools and other institutions.

Now suppose you have a definite problem, you would like to know what were the conditions in France in the 18th century or what kind of medical service did the artisan's family get in the 17th century or what kind of medical service did slaves get in antiquity? It is difficult to find explicit answers to any of these questions. So I am approaching the subject from an entirely different angle than from a sociological angle. The purpose that I am trying to bring out in the books that I am writing is to trace the history of the various great civilizations, beginning with the ancient Orient, the Egyptians and ancient Babylonians. Of course, the first chapters will be introductory, on early medicine, the survival of primitive medicine. Primitive medicine never dies. All its ideas are still to be found in popular beliefs. Then it appears in Greece, Rome, the Middle Ages, and so on, but I want to study the history of the various civilizations in their sociological structure, in their economic set-up. I want to treat the question of how they produced their food and commodities; the major health problems in the period; what was done to meet them in cures and prevention. Who were the actors, who were the leading men in this fight against disease and what did they think, what ideas were determining their actions?

I am just reversing the picture by 180 degrees. Instead of picturing it from the point of view of the doctor, I am doing it from the point of view of society. Such a book will at the same time be a history of medical science, a history of the profession, a history of hygiene and public health, a history of disease, and I think it will give a much truer picture than we had before. It is very difficult because we have to start from scratch. Hardly anything has been done, and yet I think it is very important.

In the history of science, the situation is about the same. I don't think anybody has attempted to write a similar book. In my first four volumes, I want to trace the development from the early beginnings down to the present days and then give a cross section of medicine and its social implications all over the world, the social problems, what the problems are and what is being done

to meet them in the different countries. Here I will be able to discuss questions of public health, medical help, socialized medicine, health insurance, state services, and so on, because at the present time all over the world a great deal of experimenting is being done. I am making it a large book because it is new and I felt that a new attempt should not be compressed in a short volume. It should give you the entire material in literature and footnotes, full biographies, maps, charts, and important illustrations so that through research you would be able to continue the work. That is why I am making it a comprehensive book. Later, if it is successful, it can be condensed and popularized. But first I would like to make it a scholarly presentation. My feeling is that whoever studies such a book will then get a much clearer understanding of what our present problems are. Because, after all, the situation that we are facing today is the result of historical developments. If we are aware of them we can act much more intelligently.

MR. BEAN: Dr. Sigerist, I should like to ask a question for information. You referred to Harvey's discovery. It seems to me I read some years ago that the knowledge about the circulation of the blood existed in India, about 500 B. C. or thereabouts. Is that a fact?

DR. SIGERIST: These are vague guesses only. There is no foundation at all to this. I looked up the material once. What is true is that an Arabic physician in the 13th century discovered the pulmonary circulation. Again you see that all these people did not think in quantitative terms. They said that some blood went from the heart to the lungs and back to the heart, but they didn't say it was the whole of the blood. If they had thought in quantitative terms they would have discovered the other circulation as well, because after it leaves the heart and goes through the lungs, what happens to it then?

There is a great difference between the ancient theory and the theory from the 17th century on. The ancient theory is descriptive and qualitative. From the 17th century on, people began to think in terms of quantity, to take a realistic approach and think in terms of anatomy.

DR. McCALL: Dr. Sigerist, I was impressed last night when you gave the view that a democracy was more favorable than any other form of society for the development of science. That stood out in contrast to the fact that nearly all my professors in chemistry and physics and mathematics--that would be a generation ahead of me--had finished their final degree in Germany; and certainly, whether we have surpassed them now, there was a period when Germany was outstanding

in the fields of the quantitative sciences. How do you reconcile these apparent discrepancies?

DR. SIGERIST: I didn't mean that science can flourish only in a democracy. I said that when we look at history we find that science has flourished in democratic periods. This was just one of my theses, but the development of science and the development of democracy are the results of the same forces.* Actually in the 19th century science was a development of capitalism. That was a strong stimulus to science and at the same time it led to the rise of the middle class and the establishment of democracy. I studied in Munich just before the war, and it wasn't full fledged democracy but still it was very much democratic. In the middle of the century (1848) one of the great leaders in science, Virchow, a liberal politician, published a journal for medical reform. He was a member of Parliament all his life as a liberal. They had a flourishing democratic party. They had very flourishing trade unions. Well, after all, there was an emperor there but you couldn't call it an autocracy at all.

DR. BRANDES: Wasn't Germany at that time really the heart of democracy with a small aristocracy sitting on the rim?

DR. SIGERIST: Yes, definitely. In the 40's there was a very strong democratic movement which brought down the revolution in 1845. But when you read the enormous literature published in these years, 1846-7-8, you see there was a tremendous democratic movement, which was different in different countries. East Prussia was an autocracy with a landed aristocracy. They had generals in the army. If you went to Luxemburg and Frankfurt, you found just as much democracy as in any other country.

DR. SEEGER: You know where the universities were, they were not in Prussia.

DR. SIGERIST: Naturally the people enjoyed enormous freedom in the German universities. The monarchy was proud of its universities and supported them very much.

DR. BRANDES: Dr. Sigerist, you commented last night on the fact that it was well known that certain companies had purchased patents in order to suppress them.** I don't know that I followed the implication of that, but I suppose it meant we are denied certain advantages.

DR. SIGERIST: Definitely. And I suppose that is true in every industry.

* Section II of Dr. Sigerist's lecture.

** Section IV of Dr. Sigerist's lecture. The reader will be interested in Mr. Osborn's remarks on the other side of this sub-

DR. BRANDES: Then it must be that in other countries the same thing goes on to a greater extent than it does here, because I have had the experience at times of trying to telephone people in England, Germany, and France.

DR. SIGERIST: I would say ours is by far the best. I agree with you that when you wish to call someone by telephone in Paris it is better to call up a station outside of France to give you the station in Paris than to try to do it directly. But even here we could do it still better.

A collegue of mine, a professor of anatomy in Leipzig, Spaltcholz, made studies of the skin. He described a beautiful preparation to make the skin transparent by injection into the blood vessels. You find this in almost every museum. He worked for years on this method to make the skin transparent. In making these studies he found a method to harden the skin so that he could make shoe soles that would last a lifetime. He patented it with no intention of making money, and a firm bought it from him. He wasn't interested in the matter directly. They bought it from him for very little money in order to suppress it, because there would really be a catastrophe if a shoe sole would last a lifetime. Such inventions are made all the time. Society does not benefit by them because it would harm other interests.

DR. SEEGER: Dr. Sigerist, along that same line you were mentioning last evening the infinite possibilities of science today and the limited extent that we can enjoy them. You started out to make a suggestion on a method by which society might reap these benefits. I wonder if you want to go on and tell us what we can do about it.

DR. SIGERIST: That is a very difficult problem. Whenever you attack the problem you come to the one barrier that makes it difficult for us to apply our knowledge, which is our existing social economic system. You see it in agriculture probably better than anybody else. You know what _can_ be done. And then you get to that barrier and there you are. You can produce the products but you can't sell them, at least not regularly, and it is the same everywhere.

Now, if I might say a few words, I spent three summers in Russia just because I was interested in these questions, the application of science. I made a very thorough study of Russia. My chief aim there was to study the public health system, but as that is just one aspect of the country, I had to make a study of the whole structure of the nation. I spent first of all six years

studying it. I learned Russian in order to read the publications. Then I studied all the literature I could find on the whole social and economic structure of the country, the way the state operates. Then I went three times there, in 1935, 1936, and last year again in 1938. I intend to go back about every two or three years because things change very rapidly. It is a dynamic process. If you go there once you get a snapshot, the way it is in a certain year. If you go back repeatedly, you see the changes and the trends.

We as scientists who are interested in social science cannot ignore the experiment that is carried on by one-sixth of the inhabited earth. We still call it an experiment just as the English for about a century spoke of the American experiment. What interested me there was to see the place of science in life in that section of the country. What impressed me most was that for the first time in history an attempt is being made to run a country along scientific lines. Whether that succeeds or not we shall see. I personally believe that it will succeed because I believe in the possibilities of science. The way it is done--to put it in a very few words--what makes it so interesting to work there is that whatever you see you understand. It makes sense.

I remember my first year. I had been there for four months and then went back to Switzerland to my family. At the Swiss border I bought a number of newspapers to find out what was happening in Switzerland. The first thing I read was that the Swiss agriculture was menaced by a great catastrophe, because in the summer the weather had been so good that there was going to be a record wine crop. Therefore the federal government was urged to appropriate 100 million francs to support these poor farmers who were so severely tried. I read this. It didn't make sense to me. I didn't understand it. I got another newspaper and read another record about the remarkable wine crop. I had the impression that someone was crazy, either I or the other fellow.

For several weeks I lived in a world that I didn't understand. I saw my old friends and we discussed matters but we didn't understand each other. They told me of new inventions that were going to decrease employment. We were speaking different languages until after a while I found my way back to my old world. But in Russia it was all so different. A good crop means more food and more wealth to everybody, and so on. Now why is it? It is because actually there for the first time an attempt is made to apply science one hundred percent, as much as it can be done, by having first of all redistribution of administrative districts of the country.

DR. BRANDES: I think that is a point we ought to keep prominently in mind. I suppose most Americans, or some Americans, are

interested in this vast experiment because of the idea that some part of it might find application in this country, but do you think that conditions are sufficiently similar in the United States and in Russia?

DR. SIGERIST: Of course, conditions are so different that many things could not be applied, naturally. Every government Department is doing some planning; that is what the Russians are doing, but on a nation-wide scale. We cannot do it in an all-inclusive way, but a great deal can be done through planning. In public health I think we can do considerable. I was impressed to find that what the Russians are doing is what our committee on medical care recommended, to establish health centers and give medical service, so that an individual practitioner who hasn't the help and equipment he needs can go to organized health centres where they have specialists, public health officers, and hospital facilities, all available. That is what the Russians are doing. The national report of the committee on medical care recommended just this.* There we can see how it works out; it can be done.

DR. BRANDES: Did the people pay for their medical care at all?

DR. SIGERIST: No, it is entirely free, it is part of the socialized wages of a man.

DR. YOUNGBLOOD: What do you think of the possibility of socialized medicine in the United States? It is doing pretty well in Russia; how would it work here?

DR. SIGERIST: We couldn't apply it 100 percent like it is there. I have no doubt that sooner or later we are going to get here a kind of an intermediate solution, an expansion of public service, combined probably with health insurance in those states where there are large groups of wage earners.

DR. BRANDES: In your study of socialized medicine in Russia, did you have the opportunity of talking to patients as well as to physicians?

DR. SIGERIST: Many, of course.

DR. BRANDES: Did there seem to be any indications that these low-paid physicians treated their patients in a casual or summary fashion?

* Publications of the Committee on the Costs of Medical Care (University of Chicago Press). Summerized in vol. 28 entitled <u>Medical Care for the American People</u> (1932).

DR. SIGERIST: Those low-paid doctors are the highest paid workers in the Union. They rank with the engineers and with the highest paid workers.

DR. BRANDES: Nevertheless they are low paid.

DR. SIGERIST: Well, it is relative.

DR. YOUNGBLOOD: Would the patient get the consideration, would you say, comparable to that given by our doctors here?

DR. SIGERIST: The trouble is this: they are short about thirty to forty thousand physicians, and short one hundred and fifty thousand nurses, at least. Before the revolution, Russia had twenty thousand doctors. They have increased the number now to one hundred and seventy thousand, but they still need about fifty thousand more at least.

DR. YOUNGBLOOD: Is a physician free to practice medicine according to science and not through political orders?

DR. SIGERIST: Of course. The physicians' union determines the policy.

MR. WATSON DAVIS: I just wanted to ask if they were in accordance with the ethics of the American Medical Association.

DR. SIGERIST: I have been asked about this many times. Very few doctors in Russia know the code of ethics of the American Medical Association. Whenever I ask a colleague if he has seen it, he never has. As soon as a doctor is economically independent on his salary, all these regulations take care of themselves.

DR. EZEKIEL: May we go back to your original point regarding the lag or perhaps complete lack of introduction of scientific inventions into practical use? Regardless of the social and economic system, wouldn't the problem of readjusting your new inventions be such that there would always be a lag, sometimes a long lag, between new inventions and practical application?

DR. SIGERIST: I suppose so. It depends on what the invention is. If it is new, it is a question of whether it is financially worth while or not.

DR. EZEKIEL: Even if it meant throwing away existing capital and substituting fresh capital--whether that capital is privately owned or publicly owned--you still have the economic question of the

advisability of doing it, and the problem of making the adjustment.

DR. SIGERIST: In Russia, every industry has its research institute and sometimes a number of local ones. When they make a discovery that seems to be really good, and proves to be so, then it can be applied in a nation-wide way immediately. There are no patents to prevent it.

DR. KUNSMAN: Coming back to the suppression of inventions, I am wondering whether as a general rule many inventions come out and are applied before we are ready for them. For example, take the talking movies. This was presented to the public by demand three to five years before the research work was completed. The result was noisy amplifiers and all that. It was abominable, but the people demanded it. For another example, take high-speed automobiles. Are we ready for them, judging from the type of roads and accidents? Aren't they promoted before we are ready for them?

DR. SIGERIST: Undoubtedly.

DR. KUNSMAN: If we take inventions as a whole, isn't it just about what we might expect? In some cases we are ready for them, others are held up for economic reasons at the time or put on before the groundwork is finished.

DR. SIGERIST: Yes, there are probably a good many inventions that would benefit mankind and are suppressed for definite reasons.

DR. SEEGER: Dr. Sigerist, there is one question I am worried about. Now we will say Russia is not a democracy, as a matter of definition. Then we should expect that the development of science would not be as great in Russia as it would, say, in a democracy like this country.

DR. SIGERIST: No, it isn't democracy alone that enhances science. Besides, Russia is trying as hard as it possibly can to become a democracy. It is a difficult problem over there. When you have 182 nationalities of entirely uneven cultural development, to turn the whole machinery of state development over to the people, to Eskimos and trans-Asiatic nomad tribes, that is not easy; it takes some time to become a democracy.

A very important fact is the underlying philosophy, particularly political philosophy. If you have a rational philosophy, then you have a much better condition than if you have a mystic philosophy. In the Middle Ages, or in the Fascist countries, where mystical philosophy is underlying, it is difficult, because science is after all rational. We see it in the Fascist countries very clearly. Of course, America, like Russia, is based on a rational philosophy.

The American state in the 18th century of enlightenment, the Russian state in this century of Fascism or Naziism, are both rational. In both it is the individual that counts. What justifies the state is that the state cares for the individual to promote the welfare of the individual. In the other countries it is different. There the goal of the state is some mystic goal. In Italy it is the reconstruction of the Roman Empire, in all its grandeur. In Germany it is a superior race to redeem the world. The individual doesn't count. He must merge himself into the goal of the state.

Now, if they hadn't one very practical consideration, science, and medical science particularly, would go to pieces. As one of the scientists in Germany said: "The goal of science is to produce armaments and substitute products that the country cannot have. There is no other justification for science." You see, it is not the welfare of the individual that counts, it is to reach the mystic goal of the state. When you have such a philosophy, then science has a hard stand. Where you have the rationalized philosophy there science will have much better chances. Then, of course, a country like this one where the whole economy endeavors to be scientific, science will have the best possible chance.

DR. ENGLUND: How about individual freedom under such a plan?

DR. SIGERIST: I think there is one tremendous difference: In Germany and Italy people will tell you"democracy is rotten and ought to be abolished; we need totalitarianism." In Russia they always apologize for whatever dictatorship they have. They say, "Yes, we still have dictatorships. We still limit civic liberty. We are sorry for it and we hate it but we can't afford yet not to do it." I think that after all there is a basic difference in attitude. When you see the changes from year to year as I observe them, you see an honest endeavor to come closer to the democratic goal as soon as possible. What the Russians always told me was that you in America have had democracy for 150 years' time and yet your Indians have no civic rights, there are negro sections in the country where they have little or no civic rights. It is not a democracy. In 150 years you have not been able to do it; how can you expect us to do it in 20 years with our Eskimos and nomads and these 182 races. I think there is something to it. Of course the underlying theory is all for democracy. They say the ideal democracy is possible only where money does not give political power. Now, of course, whether twenty years is a long period of time or not, we have to find out.

MR. BEAN: I want to go back to a little different kind of problem. You emphasized the importance of understanding and knowing history as an aid to understanding the present. Would you go so far as to say that a thorough understanding of history and the present is of any use in anticipating the futher?

DR. SIGERIST: It is always difficult to be a prophet; the subject is so complex, so much more complex than in a science where you have a smaller number of factors to consider. When you make a scientific experiment you have a number of factors. But in history there are so many more factors that come in that it is difficult to make predictions, yet I think, particularly in the Marxian school, they made predictions a hundred years ago that are amazingly true.

DR. SEEGER: Are there laws of history?

DR. SIGERIST: Well, yes, there are, to a certain extent I think. They are not quite so clear-cut as the scientific laws but there are certain laws you can expect. One would be that, for instance, when people are hungry there is a tendency for them to complain, and be ready to fight, which means a great deal in history, of course.

DR. THONE: Could we say there probably are some rather definite laws but we don't know them yet?

DR. SIGERIST: Yes.

DR. DEMING: Dr. Sigerist, yesterday you talked about science and democracy being actuated by the same forces, not that science thrives in a democracy but rather that they spring from the same roots.* In view of the discussions that we have had here previously, and in view of some of the questions that have been asked this morning, I think it would be well if you would repeat what you said, or clarify it a bit before we close.

DR. SIGERIST: I was thinking particularly of this: the coincidence of the great scientific development with the development of democracy in the 19th century is, in my opinion, largely due to the development of the capitalist system of production by the middle class. This rising and developing middle class trying to apply the new system of production led to the development of democracy, the overthrow of feudalism, and at the same time created a tremendous development of science, because developing industries needed science. Whether it will be so in the future, I don't know.

DR. YOUNGBLOOD: Alexander the Great gave great freedom of thought and action to his scientists, didn't he?

DR. SIGERIST: Yes, surely. That is how an enlightened monarch or an enlightened despot of any kind may promote science tremendously. In the court of the Ptolemys at Alexandria science

* Cf. Section II of Dr. Sigerist's lecture; also p. 124 of this seminar.

was supported by the kings and the royal families as never before in ancient history. Peter the Great did it in Russia. This was a barbaric court, yet he established the Academy of Science that has existed ever since and has done great things.

DR. YOUNGBLOOD: Of course democracy doesn't guarantee all the truth; it is like telling children the truth. Custom and tradition stops us short of the facts, like Tennessee when they voted the Scopes bill. There is growth in freedom.

DR. SIGERIST: Of course. You couldn't teach evolution in certain states. There you have it. It is a democracy but sometimes it is a nuisance to science.

DR. SEEGER: Do you believe then that the great growth of science through Galileo and Harvey and the rest of them was an outgrowth of the economic conditions at that time?

DR. SIGERIST: Yes.

DR. SEEGER: It always seemed to me another factor very vital was downright curiosity.* The Greeks who were seafaring were curious and it was that same curiosity that gave rise to science.

DR. SIGERIST: Yes. The discovery of the world makes people curious to know more. But then, of course, the fact that the Greeks were seafaring is an economic factor. They were a nation of merchants. That is why they were seafaring.

DR. DEMING: I am afraid we have to stop here. We have reached the hour of closing.

* The reader may recall the discussion on this point in the first seminar, pp. 2-4, 14-17, 23.

Ten Lectures and Discussions on

SCIENCE: Its History, Philosophy,

and Relation to Democracy

held at the Department of
Agriculture Graduate School
under the chairmanship of

M. L. Wilson
Under Secretary of Agriculture

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Edited by W. Edwards Deming
Assistant Director of the Graduate School

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Lecture VI

TO WHAT EXTENT IS A SCIENCE OF MAN POSSIBLE?

by

Frederick Osborn

Research Associate in Anthropology
American Museum of Natural History

The Graduate School
The Department of Agriculture

Washington
1939

TO WHAT EXTENT IS A SCIENCE OF MAN POSSIBLE?

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Frederick Osborn
Research Associate in Anthropology
American Museum of Natural History

In the auditorium

In introducing the speaker, Under Secretary Wilson said, "Thus far, our speakers in this series have been chosen from the fields of biology, physical science, mathematics, and philosophy. Our subject this afternoon is 'To what extent is a science of man possible?' and our speaker, Mr. Frederick Osborn, is a Research Associate in Anthropology at the American Museum of Natural History in New York. For a number of years he has been actively engaged in research relating to human genetics and population problems. His address 'Science and Society', which was delivered at the Indianapolis meeting of the American Association for the Advancement of Science (December 30, 1937) received widespread favorable comment, and we are very happy to have Mr. Osborn with us this afternoon."

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I

Knowledge of man has been growing slowly over thousands of years. But a science of man is something new under the sun. For though science is knowledge, it is a special kind of knowledge. It is obtained by scientific methods, usually involving a collaboration between theory and experiment. Most science is based on the quantitative analysis of measured phenomena. It differs from other knowledge chiefly in its quality of being demonstrable. An experiment to have scientific value must be one that can be repeated. Scientific phenomena can be measured and recorded over and over again, or related by theory to other phenomena that can be repeated. New knowledge of this sort becomes generally accepted when it has been checked over by a sufficient number of people. The older type of knowledge which is derived from personal observations and the conclusions of authorities is harder to check up on, is more subject to personal bias and the mental fashions prevailing at any given time. Scientific knowledge, on the other hand, is cumulative in its effect and has a known predictive value.

In a hundred thousand years, by his use of the old forms of knowledge, man developed an environment suitable for a civilized life. He domesticated animals, produced cereal crops, and through the great religions aspired at least to a noble concept of the dignity and character of life.

Then, in a few brief generations, the new forms of knowledge which we call science brought to men a marvelous control of their environment. Railroad, telegraph, telephone, electric light, motor car, submarine, aeroplane, radio, television, reduction of labor needed on the farm, canned and frozen foods, cheap goods by mass production, sanitation, medicine, and public health. Almost over night the natural and physical sciences have brought these changes.

The biological quality and the training of man has not undergone comparable changes. Except for medicine, we know so little about man that it is still fair to ask, 'Can we have a science of man?' And the question is, somehow, troubling. Man's new power to control the environment has not made him humble. He seeks new short cuts to happiness. The older knowledge of man, heritage of ages of experience and suffering, he tends to discount, because it is not based on the new scientific method, not capable of scientific proof. He is not likely to go back permanently to the old knowledge. He is too impatient of its restraints, too admiring of the success of the new type of thinking based on the scientific method. Yet without more knowledge of himself, of his needs, his weaknesses and his possibilities, we may wonder whether man can safely handle the extraordinary tools he has recently created. They may inflict irreparable injuries. If we ask, 'To what extent is a science of man possible?' perhaps we are really asking to what extent can we achieve a secure and permanent civilization?

It is hardly encouraging to compare the present state of the science of man with the marvelous development of the natural sciences. But the picture is a more hopeful one if we make allowance for the respective ages of these two fields of science. Several generations of men have been trained and taught in the physical sciences. But no one of the age of forty-five or more today could have had any serious training at college in the sciences that have to do with man. They were not available for teaching twenty-five years ago, which is a pretty brief span of time, even in this hurried age.

Scientific work in psychology was in its infancy at the turn of the century. Mendelian genetics were rediscovered in 1901. At about the same time anthropologists got out of their arm chairs and began collecting ordered data in the field. By 1910, textbooks were beginning to make significant use of new scientific materials in these fields. By 1920, courses in scientific psychology, genetics, human biology, and anthropology were available in most of our universities. Today these subjects are among the most popular of any that are offered. But much that is taught about man and society is not science. Not enough research has been done to supply the basic material needed; and, still more important, there has been too little time for critical analysis, interpretation, and organization of the research that has been already carried out. Notwithstanding this present handicap, the sciences of man have already begun to influence our thinking in a way which suggests the effect that they may have in the future when they are more fully developed. A few examples will make this clear.

Psychology has made important contributions to present day points of view. There are some two thousand registered psychologists in the United States today, where there were only a few scattered individuals in 1900. The sum total of their research fills innumerable volumes. But much of this research has been badly done, as would be expected in so new and difficult a field.

Some of the most important work in psychology has been that on the development and experimental application of psychological measures, which are the necessary tools of acceptable research. Methods of measuring intelligence have been used extensively and criticized and refined during the last twenty-five years. Their weaknesses and limitations are now fairly well recognized and understood. In the hands of competent psychologists they have given us what little scientific knowledge we have concerning the origin and development of the faculty of intelligence which most distinguishes men from other forms of life.

There is still controversy among psychologists; part of this may be described as a controversy between older schools of psychologists and those trained during the last decade. Laymen who engage in controversy are often found to be leveling their lances against concepts and methods that have been completely discarded by critical contemporary psychologists. Much of this controversy relates to the roles of heredity and environment in the development of intelligence. Recent work goes far to clarify this difficult field. In the past few years, the so-called fixity of the I. Q. has been disproved. We know now that a stimulating environment in the home, in pre-school, in elementary school, in high school, in college, and in later life, tend to raise the I. Q. of an individual, and to maintain it at a higher level. We know that in a depressed environment intelligence fails of a normal growth. A child of two with apparently normal intelligence may in the unstimulating surroundings of a badly run orphanage revert to feeble-mindedness. On the other hand, there are important differences between individuals in the extent to which they respond to the stimulus of the same environment. Individual differences do not disappear when the environment is equalized at a high level. In a stimulating environment, able individuals show a capacity for response which takes them further than ever out of the class of those of average ability. Among Newman's 19 pairs of identical twins reared apart, there were 11 pairs in which the two members of each pair had had similar amounts of education. In each such case the twins differed in I. Q. only about as much as the same individual would vary when tested at different times, the average of differences being 4.4 points. Among four of the pairs there was considerable difference in schooling between the members of each pair; their I. Q.'s differed on the average by 10 points. Among the four remaining pairs, educational differences between the members of each pair were large, and in these four cases the twins differed by 19 points, on the average. In every case the twin with the more education had the higher I. Q. But at the same time where one twin was dull for his poor environment, the other was dull for his good environment, and where one twin responded well to his poor environment, his mate responded well to his good environment.

Twin Eleanore only got as far as the fifth grade, and attained an I. Q. of only 66. Her sister Georgiana went through grade school, high school, four years of music, and three years of normal school. After all that education, her I. Q. was only 78. It is hard to escape the conclusion that this pair pf identical twins were not endowed with the genetic factors necessary to ordinary intelligence.

Twin Gladys, with only three years' elementary schooling as the total of her education, had the creditable I. Q. of 92. Her sister Helen, with a college degree, had an I. Q. of 116. Evidently the genetic endowment of these girls was sufficient for the development of average intelligence.

The findings on identical twins reared apart check pretty well with other studies on the relative contributions of heredity and environment to individual differences in intelligence in the general run of our population. Of course, nineteen pairs collected by Newman, and one by Muller, is a number woefully inadequate for statistical validity, but this inadequacy is typical of the present state of the science of man.

If heredity is indicated as an important factor in differences in the intelligence of individuals, this is far from being the case with respect to differences in average intelligence which are found between socio-economic or occupational groups. There have been five studies of foster children in which it was possible to distinguish the occupational or socio-economic grouping of their true parents. These are the studies by Burks of California, by Freeman of Chicago, by Leahy in Minnesota, by Skeels in Iowa, and by Lawrence in England. In the Freeman and Skeels studies there is evidence of selective placement. That is, the brighter children were more often than not placed in the superior homes. In the other studies this was apparently not the case. The findings from all five studies were similar. The children whose true parents were in the lowest occupational or socio-economic groups had an average I. Q. about 6 points lower than that of the children whose true parents were in the upper occupational or socio-economic groups, the foster homes being in all cases carefully matched for educational stimulus. This difference of about 6 points indicated as the contribution of heredity is small, and becomes even less significant on further analysis of the methods necessarily employed in these studies. If it had been possible to set these studies on a better basis, it is likely that an even smaller difference would have been shown. If these are the indicated differences between the relatively small group at the bottom and top levels respectively, then the differences in hereditary capacity between any of the larger groups are of little importance. As in the case of identical twins, the number of studies is quite inadequate for final conclusions. But even these tentative findings point to some interesting inferences.

If innate differences in intellectual capacity are on the average so small between different occupational classes, then our educational system should not be permitted to become a class affair. The American ideal that would open the highest educational opportunities to young people from every rank of society would seem fully justified. On the

other hand, if hereditary factors are of major importance in determining differences in intelligence between individuals in a similar environment, then our educational system should be pointed up to meet the needs of individuals at different levels of genetic capacity.

Thus, the recognition of individual differences shows the need for the separation of the sub-normal from the intellectually superior in their class work if either group is going to benefit fully from its education. The beginning of such a separation is being made in some schools today. It is one of the important contributions made so far by the budding science of man. This process of fitting the education to the capacities of the individual will undoubtedly continue as our educational system becomes increasingly affected by our growing knowledge of individual differences in capacity.

III

In the field of genetics, the marvelous advances of the past forty years have been largely limited to the genetics of plants, insects, and animals. For some reason, human genetics has been largely neglected in this country compared to what has been done by Fisher, Haldane, and Hogben, in England, and by Verschuer and others in Germany. There is almost no knowledge of genetic factors in normal variations in general qualities, such as intelligence, character, or susceptibility to disease. Important work has been done on blood groups. A considerable number of infrequent abnormalities are known to be due to genetic factors and in some cases the mode of inheritance is known. Research work on genetic factors in feeblemindedness and in mental disease is almost all in the future. Nevertheless, there are many signs of an aroused interest in the medical profession and a new recognition of their responsibility for preventing the spread of serious hereditary defects.

Ultimately, scientific knowledge in regard to the part played by genetic factors in causing individual differences, and further research on the inheritance of different genetic factors, may make possible measures that would tend to discourage the reproduction of inferior genetic strains and encourage the reproduction of those above the average.

Thus, scientific knowledge of the relative parts played by heredity and by environment in developing individual differences may become a valuable tool for improving human qualities, first on the environmental side through changes in education, and ultimately through raising the average hereditary level.

IV

Anthropology has made at least one important contribution to the American point of view by showing the extent to which culture patterns are fixed by the social environment with little regard to the type of people involved. Many of the qualities commonly spoken of as 'racial characteristics' are now known to be matters of social rather than

genetic inheritance. The whole concept of race has undergone a violent transformation in the past fifteen years. It is said that Hitler during the two years he spent in jail before coming to power read widely in what were then supposed to be scientific books dealing with race. They were not scientific in our present definition of the term: they were the German analogies of Madison Grant's Decline of the Great Race, which was having a vogue in this country at that time. Modern research of a more scientific sort denies most of their conclusions. It is interesting to speculate on what would have been Hitler's attitude towards race if he had had access to present scientific knowledge.

These few and tentative conclusions are suggestive for the future, but they do not indicate to what extent the science of man may be possible. It is only very recently that we have really lifted age-long taboos against an honest examination of ourselves. In the past twenty years more of a start has been made than might reasonably have been expected under the circumstances. The prospect for the future seems hopeful. The extent to which we can have a science of man would seem almost unlimited provided three major conditions are met:

The first is plenty of time.

The second is the enrollment in this work of men of high abilities, with adequate support.

The third is freedom of thought, freedom of inquiry, and freedom of criticism.

V

Research problems concerning man are, in many cases, no different in kind from research problems concerning other forms of mammals on which effective work has been done. But the problems of man are infinitely greater in complexity and require more time in proportion as the space between human generations is longer than the space between the generations of the smaller mammals. What Tryon learned about the genetics of maze-running ability in rats might be duplicated in human beings with regard to genetic factors in differences in general intelligence, but it would take 200 years and a quite inconceivable control of human breeding to carry out such an experiment. The difficulties of studying environmental influences are almost as great, but there is no reason to believe they cannot be solved by sufficiently persistent effort and by the development and application of new methods.

There remains one important difference between the study of animals and the study of man, namely, that in the latter case man is studying himself and thus finds it more difficult to exclude his personal and emotional biases and reactions. It is for this reason, among others, that freedom of criticism is as important as freedom of thought in the development of the science of man. With all these difficulties

taken into account, there is still every reason to believe that the development of the science of man will go forward rapidly from its present modest beginning.

VI

It is worth while to consider the different practical applications that may result from the sciences of man. The first has to do with education. Present methods of education are the product of a long evolution under the guidance of the old type of knowledge. On the whole, education today is undoubtedly better then the education available in the past. But we do not know in any precise way what a modern education really does, or the different effect it has on different types of people. Psychologists in great number are working on new measures for determining individual capacities along different lines. Other psychologists are trying to determine the effect of different educational environments on people of different capacities. We may be sure that there is no single environment that would be the optimum environment for everyone. Each individual will make his maximum development in the environment that will most stimulate the particular responses of which he is capable. The environment that would be optimum for a dull person would be insufficient for the full development of a superior person. In the studies on orphanage and pre-school children being made by Stoddard at the University of Iowa, the brightest children showed the least growth in the deprived environment of the orphanage. The Pennsylvania Enquiry on school and college education by the Carnegie Foundation indicates wide individual differences in ability to respond to a college education. A considerable proportion of those going to college go backward rather than forward intellectually during their last four years of schooling. It is not too much to hope that work of this sort will develop a science of education such that ultimately we shall be able to measure the specific potentialities of each individual and provide an educational environment which would be the optimum for each of his particular abilities. Such a change in our educational system if universally applied would probably raise the average I. Q. almost 20 points. Few people would remain without some specific capacity which, properly developed, would make them more valuable members of society in their own recognized specialty.

With respect to the development of character and a balanced personality, scientific advance is more difficult than in the field of intelligence, but work is now going forward which will hasten the revolution in our treatment of criminals, and which will ultimately greatly reduce the number of anti-social, distorted, and unhappy personalities; and even for the normal child will make possible a more socially adapted and happier personality.

VII

To date, the most effective applications of a science of man have been in medicine, nutrition, and public health. The expectation of life at birth is now double that prevailing a century and a half ago, and has been increased from 49.2 years in 1900-1902 to 60.3 in 1929-1931. Medicine had a long start on psychology. It is not unreasonable to suppose that in another fifty years we may have a science of man which can prescribe the optimum environment not only for the maximum physical but also for the maximum intellectual and personality development of each different individual. The application will not be easy. But when the knowledge is available, some way will be found to apply it.

The science of human genetics will ultimately supply psychologists with additional knowledge necessary for an understanding of different human types. But the major applications of the science of human genetics will be in the field of direct improvement of the genetic qualities of human stocks. That is far in the future. What a few men did in twenty years, working in Drosophila, it may take several hundred men a hundred years or more to do working on man. Given sufficient time, and the development of new tools of research which will surely take place, a fairly complete genetics of man is possible. Whether its practical applications will be important, I leave to you to decide. Does your experience in plant and animal genetics lead you to think that the average man's socially valuable qualities in our changing environment could be improved by creating conditions in which superior strains have the larger families, and in which the breeding of inferior strains is effectively discouraged?

VIII

We have been considering only the sciences relating to individual differences and individual development, and the contributions which these sciences may make to the improvement of human beings. But the study of man cannot proceed independently of his environment and his activities, which are the field of the so-called social sciences. Nor can the social sciences proceed successfully without more knowledge of this strange and complicated creature, man. The development of the science of man should therefore have another important effect in the contribution it will make to the sciences which deal with the behavior of men in the mass and their relations to each other.

All branches of sociology are at present handicapped by lack of knowledge of the human material whose activities they are studying. The postulate of the economic man, impervious to all other emotions, does not add to the reality of economics. Perhaps the new field of population study provides the best example of the interdependence of studies of man and studies of man's activities. Here the analysis and forecasting of total population trends has been revolutionized since 1925 by the introduction of procedures for taking changes in age and sex composition accurately into account. It is safe to say that the error in population forecasts for the United States for the next thirty years has been cut in

half by the application of these techniques, and equally important information about future age distribution has been added, which was wholly lacking before.

The study of the adjustment of the population to resources in different parts of the nation, which had never been given serious attention before 1930, has been developed to the point where its results already have very practical and far-reaching significance. The study of differential reproduction rates, which prior to 1930 had been based chiefly on such fragmentary and inaccurate data as reports by college students about the numbers of children in their fathers' families, has been extended and refined until it is possible to describe the reproductive tendencies of most population groups in the United States with considerable accuracy, and we are now beginning to get accurate information on how these rates are changing in different groups under different conditions. It may be hoped that the 1940 census will supply the necessary materials for far more accurate and extensive studies along this line.

But these so-called group differentials in fertility relate only to occuptional groups or with regional groups. We know that farmers have more children than city people, but we do not know what genetic types are surviving in the greatest numbers. It is impossible to say at present on the basis of any scientific evidence whether the human race is improving, or whether it is deteriorating. This important question, with all its practical implications, can be answered only when population study can employ measures of innate human qualities, and for these it must wait on the development of the science of man.

Thus, the science of man may not only make it possible to improve man himself by supplying the proper environment for his development and, ultimately, even by an improvement in his genetic potentialities, but may give the social sciences sufficient precision to make them truly sciences capable of predicting the end results of current political, social, and economic trends.

The sciences of man may in these ways make an invaluable contribution to human welfare. They may also make an even greater contribution in setting up new concepts of human possibilities, new ideals as to the purpose of life.

IX

In the long period of comparative world peace which lasted 100 years from the Battle of Waterloo to the outbreak of the World War, there seemed to be a growing recognition that the physical well-being and the fullest intellectual and moral development of individuals was the highest goal of society. This ideal of individual improvement was eclipsed by the old barbarism which emerged in the war and by the desperate struggle for power that has followed. We seem to be going back to primitive aspirations in which the improvement and happiness of the

individual are subordinated to the power of the state. A return to religion would prevent this trend going further, but a new stimulus is needed. That stimulus may lie in the science of man. When we have a clear picture of what an optimum environment can do for the development of the individual; when we understand better the extent to which inferior genetic qualities may limit human development compared to the full flowering of those with a superior inheritance; when we realize that for each region of the earth and for each stage of its civilization there must be an optimum population at which its people are best supported; when economics and sociology understand the human material with which they are dealing and become in their turn sciences of human relations; when we have in our hands demonstrated scientific tools capable of vastly raising the level of human relationships and of the developed qualities of man, may we then not hope that men will be moved to new aspirations commensurate to their new possibilities? Science will never take the place of religion in revealing the ultimate mysteries and purposes of life -- whence we came, our purpose here on earth, or what lies beyond. But religion is also concerned with improving the lives of human beings, and hence religion will be strengthened by the sciences of man as they demonstrate methods for attaining a new and higher ideal of man's life on earth. And the demonstration that such an ideal is possible is the first step towards attaining it.

SEMINAR

following the lecture by Mr. Osborn
Office of the Under Secretary

Syllabus

1. It is difficult to control enough variables in social studies; pp.143-5.

2. The scientific attitude leads to tolerance; p. 145.

3. Through studies in the science of man we should be able to define the type of environment that is best for each individual's fullest development. Pp. 146-7.

4. The science of man will require time; p.148, also section IV of the lecture. The study of sibs may enable short-cuts to be made; p. 149.

5. The improvement of man should not be confused with the improvement of his environment. Man must be studied as an individual with personality, and with recognition of genetic factors. It is not enough to study his physical and social environment. Pp. 148, 157.

6. Man's emotions must be taken into account in any calculations that involve his behavior. P. 149.

7. Old family records are not sufficiently objective to be of much use in the science of man. Pp. 149-50.

8. The medical, nutritional, and public health sciences of man are in an advanced state, but in genetics and psychology there is not much that we can tell the public safely at this time; pp. 150-4.

9. What institutions ought to do research in the science of man? The work of the Department of Agriculture is involved in the science of man. P. 151.

10. Immature findings should not be broadcast but should be examined and checked over in the normal process of education. Pp. 154-8.

11. Is not the time ripe now for a widespread interest in the science of man? P. 155.

Proceedings of the seminar

MR. WILSON: I have been feeling very good since Mr. Osborn's lecture last night. If we are going to make this world into a better home for men to live in, and if we are going to use science to do it, as I think we must, I find a great deal of encouragement in the talk that Mr. Osborn gave us. I was much impressed with his outline concerning the status of what might be called the science of man at the present time, which he related with remarkable scientific modesty and yet with a very great hope. It wasn't science without any backbone, it was science that had a hope and a future to it. And I was also impressed that he didn't stop with science here, but, if I interpreted him correctly, he wound up on the note of valuing the place of philosophy and the place of religion; and that if science is to grow, philosophy and religion also must grow. He raised in my mind a great many questions that I should like to talk with him about, but he left me full of a great deal of hope for the future.

We sometimes ask questions of the speaker, Mr. Osborn, not with the idea of arguing with him but to be sure that we clearly understood his ideas. Then very frequently we have ideas of our own that we want to throw into the discussion, but really what we try to do here is for each fellow to think his own way through. Very often a person has an answer to his own question, or else he wouldn't ask the question. So remember you always have a right to throw it back. One question that I know must be in the minds of a good many of us is this: what effect will this developing science of man have upon the so-called social sciences, and more particularly upon the action or policies that are to grow out of the social sciences?

MR. OSBORN: I must begin my answer, I think, by raising a question concerning the social sciences. I can't help but think of science, as I am sure you all do,as products of the scientific method. A method of study in which the variables are controlled. I don't think to date it is possible to control enough of the variables in the so-called social sciences to make them really sciences in the ordinary sense of that term. It is probably always going to be difficult to control the variables in economics and sociology. The value of an experiment is decided by your ability to repeat the experiment and get the same results. Here the chief variable is man himself, and my hope is that the development of more knowledge of man will give us an increasing experimental

control of that variable, and that as we develop that knowledge the things that I prefer to call the social studies will become more scientific in their nature. If you have a number of uncontrolled variables, and you get the same results even when those variables are different, then it is evident that the results of the experiment are due to some of the variables that you are controlling.

DR. DEMING: And you have elicited the information that the result is independent of those you didn't control, which means a great deal.

MR. WILSON: Well, answering my own question, Mr. Osborn, I think as developments take place over here in the various sciences that you outline, they will take effect pretty quickly in the thinking and attitudes that go on in the field of government and human affairs. I thought that the illustration which you gave* (new to me) of Hitler having read the Madison Grant kind of literature, which has been shown in the last fifteen years to be a scientific fallacy, was very interesting. And now, growing out of the new scientific ideas, you have ideas of tolerance that are completely contrary to the ideas of Hitler. This more tolerant attitude is based on something that is more scientific.

DR. SEEGER: Mr. Osborn, did I understand you to say last evening that we have been going down hill from 1914 on?**

MR. OSBORN: I think that the so-called liberal concepts which were developed to a very large extent during the 100-year period from 1814 to 1914 have been greatly weakened. Certainly throughout Europe the idea that the state exists to provide the greatest happiness and development of the individual is less strong now in a great many countries than it was, and that idea has been discarded for the notion that the state itself is the important thing and that the object of the state is to aggrandize itself, perhaps even at the expense of the individuals of the state. Mussolini talks about the grandeur of Italy as if it were a being to which all the people of Italy must be ready to contribute their lives, if necessary, so that the state will be great. That is a different conception from the American, the liberal English, or even the liberal Germans of the pre-war period.

DR. SEEGER: You mean that such knowledge of science as we have is not being applied at the present time in Eurpoe for the best welfare of the people. You see that would immediately raise this question in my mind; suppose we had a science of man, suppose we had this knowledge, have we any way of knowing that it would be applied for the best welfare of the people?

* Section IV of Mr. Osborn's lecture. ** Section IX of Mr. Osborn's lecture.

MR. OSBORN: It seems to me that in supplying a clearer picture of what might be possible in the way of individual improvement, science would be giving us new values to reach for. The sciences of man should be able to define for us the different types of environment which in the case of any given individual would provide a well balanced personality, for the maximum development of his intelligence, and his maximum physical development--in other words, the environment that would be best for each individual's maximum fulfillment of happiness, interest, and activity. If science demonstrated that under proper conditions each of us or our children could reach that maximum, it seems to me that we should be inclined to say, "Well, those are the things we care about. That is what we want this government of ours to do for our people," and we should be less inclined to say, "We have to attain happiness by conquering Czechoslovakia," or what not. We should be skeptical of other plans. We should say, "We have a picture of the happiness that is right within our hands; let's go out and try to attain it."

DR. ENGLUND: Mr. Osborn, these terms "happiness" and well-being" which we set up as aims and objectives towards which we strive, are themselves rather elusive, in that they presuppose a certain standard of values. When the great liberalism of the past century was unfolding itself, we had a standard of values also unfolding itself in which liberty was an end in itself. Patrick Henry said "Give me liberty or give me death." Liberty ranked very high in his bundle of values. Now suppose, however, that a standard of values of a people changes, that they begin to feel that liberty has not achieved certain things which they had thought were achievable, let us say economic security for example. When their bundle of values changes so that people begin to think that liberty is no longer an end in itself but is good if it achieves good things, then when our concept of the good changes, what becomes of liberty?

MR. OSBORN: You are quite right to check me up on the use of such a broad general word as happiness. I was using it in my own peculiar sense and that is this: if we think of man, as we can for a moment for the purposes of this discussion, as a mechanism which has been developing over a great period of time, we think of that mechanism as having a great variety of functions and abilities. We can run, throw a baseball, we can use our minds, we can eat, and so on; all the different functions which have been developed over this long period of time have given us our survival values. Now, having been developed with all of those functions, I can imagine that our emotional internal balance derives satisfaction from functioning. It seems to me that it is in the nature of a living organism to reach a harmonious balance when it is fully functioning, and that a proper definition of happiness would probably be a definition

that would signify an internal satisfaction due to the normal balanced functioning of all the parts. Some day in the future it may be possible to determine by chemical or electrical analysis whether a person is happy, and it is that kind of happiness that I had in mind. It is a scientific kind of happiness and not dependent on any large word such as liberty or whatnot. It is that kind of a picture I have in mind. I think we must associate happiness with functioning. If we think of a happy dog, we don't think of a dog lying down, we think of a dog chasing a rabbit. Does that clarify my rather careless use of the term?

DR. ENGLUND: Yes, I think it does, in my own reaction to it. Of course I agree wholly with you even with the proper use of the term happiness, but I am thinking in terms of other ideologies. A dictator asserted the other day that it is pure humbug to suppose that men want to govern themselves. Now, suppose you had a human group that actually didn't want to govern themselves. Then the liberty of which they are deprived when somebody dictates to them doesn't inhibit the happiness of that particular group. That is why I wanted to get a clarification of the idea.

MR. GLICK: It seems to me that the most powerful social organizations are ofttimes based on something that is quite irrational, and that the scientific attitude is diametrically opposed to such organizations because the scientist is critical of everything. Consequently, if you want a powerful organization, such as Germany wanted, you must stamp out the social sciences because they are critical of everything, of all irrational patriotism. Science in such a nation might be harmful, according to such a standard. If you want to develop that sort of state you have to cease to be critical, and build a patriotic type of people. I wonder if there is an inconsistency between the province of the scientific attitude and the province of something that is quite irrational but powerful.

MR. OSBORN: I think there is a great deal of inconsistency at the present time, because the sciences of man are so undeveloped that they haven't given us clear concepts, nor methods of attaining those concepts if we had them. But it is conceivable to me that if we had a fully developed science of man so that our concepts were perfectly clear and so that the methods of attaining those ideals were justified by science, then it might be possible for a Hitler to say "This is what all men want and this is the method of attaining it, as generally agreed by a couple of centuries of scientific research. All scientists agree that this is what mankind wants in order to be happy, and these are the methods of attaining it. I will take charge of setting up the state under which you approach these ideals." Things might work that way; I don't think that is the way they will work, but it is hard to say definitely that science is incompatible with

any particular form of government, because these things are in such a state of flux.

DR. BLAISDELL: Do you see any similarity between the present time with its optimism regarding the possibility of developing a science of man, and the period in history 150 years ago when natural science was just in its beginning and rationality had great vogue, and the intellectuals were seeing the future in rational terms?

MR. OSBORN: Well, I should be rather surprised if many people share what Mr. Wilson has kindly called my optimism about the science of man. It seems to me generally that this is a period of considerable disillusionment about science, but I am optimistic about the possibilities of the science of man, though I am not at all sure that we are going to have the time and sense to achieve it. The things that I hope for from a science of man can't come for quite a while. Whether we can survive that period and gain a real science of man and then be guided by it, I just don't know.

DR. BLAISDELL: It might be that we have already passed through that optimistic period and gone into the period of romanticizing certain ideals. Possibly the drift away, the retreat from reason, as Hogden calls it, is in itself a parallel to the romantic reaction from the rational era.

MR. OSBORN: Yes, I should think so. But it seems to me that in the period of the hope that all of the problems of the world were going to be solved by scientific people, the improvement of the environment was confused with the improvement of man himself. When I was a boy (not so very long ago, as those things go, speaking in geological terms, Mr. Wilson) I thought of the improvement of the human race in terms of the improvement of its environment. I thought if we could provide a perfect physical environment for man, everybody would be happy and we should all be improved. It wasn't until much more recently that I realized that the actual improvement of man himself was a very different matter and depended on very different things. I think with the attainment of this remarkably successful control of our physical environment, we have been disillusioned with the results on man himself and on his aspirations and development.

DR. WAUGH: I have been wondering what Mr. Osborn meant when he said man is the principal variable. Perhaps I can illustrate my question this way. We know that during the last thousand or two thousand years, any period you take, social and cultural phenomena have varied greatly; forms of social control, duties, opinions,

customs, and what not. Does that statement of yours mean to imply that man has varied even more than cultural phenomena have varied during these thousand or two thousand years?

MR. OSBORN: No, I don't think of man as a variable in that sense. My own feeling is that the basis genetic qualities of any large group of people would be pretty similar to those of any other large group of people.

DR. WAUGH: In that sense man would be the constant rather than the variable.

MR. OSBORN: Yes, in that sense I think it would be fair to say that man would be the constant, but I was thinking of man's variability in the sense of the behavior that we might expect of him. We might say in economics that man will buy in the cheapest markets. That might or might not be so; for some unexpected emotional or psychological reason he might <u>not</u> buy in the cheapest market. He might think a certain kind of food wasn't good for him. He might be entirely wrong; it might be the healthiest food there was; but for some emotional reason he would not buy it. That is the case in large sections of the South. Up in Millbrook where I occasionally ride to hounds, we have a huntsman from Virginia. After he had been up there for about a year he came down sick, and after having a great deal of trouble the physician found he had pellagra. They investigated why he should have pellagra when he was drawing a good salary in Millbrook, and they found that he had been eating food that he had ordered from Virginia. It consisted solely of corn pone and ham, and that was all he was eating. That particular man is an illustration of the variability in the behavior of man. If we knew the extent to which these attitudes were due to conditioning or to some genetic factor that makes the conditioning of one group different from the conditioning of another, then we should be that much nearer the solution.

DR. WINTERS: You spoke of the limitations in the study of man, particularly genetics. In the case of identical twins we are supposedly limiting the heredity and studying the influence of environment. We are of course limited in the number of identical twins that are available. Do the nations that have kept careful family records have an advantage in the study of family lines? Some nations have had family records for centuries. Are these records adequate for the study of genetics?

MR. OSBORN: On the whole I have to answer no, because there are no records that carry with them a sufficiently exact clinical diagnosis, and it is probably necessary to scrap all of our old family records because the diagnosis is too inexact. The work which is being planned now in

human genetics of that sort requires a clinical diagnosis by a physician or a psychiatrist using some generally accepted or agreed bases of measurements. Now, if that type of record can be kept for a number of generations, and the same measures can be used, so that they are comparable, then the family record would be of great value. At present, in order to make a short cut in time, work is being done on carefully gathered records of sibs. Exact records of a single generation may be more valuable than inexact records of several generations.

DR. ALLIN: Mr. Osborn, I was just thinking, suppose we had a science of man that would enable us to achieve happiness as you define it, and that the science of man included a careful analysis of how Hitler might do just the way he did. Now, what is there in the science of man that would prevent any forceful individual from using the science of man to accomplish his own purposes, other than some technique for educating the people generally about the science of man?

MR. OSBORN: Well, if I understand you correctly, you mean that a knowledge of the science of man would give one man a great control, a great ability to govern people. Well, I think that is true.

DR. ALLIN: There would be no hope for achieving that ultimate state of happiness so long as individuals are free to make use of the science of man to achieve their own purposes by hoodwinking their fellow men.

MR. OSBORN: That would imply that the masses of people wouldn't know anything about the science of man.

DR. ALLIN: The limiting factor then would be the knowledge possessed by the people concerning that science, and that brings in my next question. I hold a suspicion that we have a great deal more of the science of man already then we are practicing. The gap between our knowledge and our performance is so wide that, as I see it, what we need now is not more depth in the knowledge of the science, but wider and more general dissemination.

MR. OSBORN: I don't feel that the gap is so wide. The only science of man that is in an advanced state is the medical, nutritional, and public health sciences of man, which are in quite advanced states. We really know something about those things, but when it comes to geneties and the psychology of man, we really don't know very much which we can tell the public safely. Most of the things that we tell the people in psychology are incorrect, I think.

DR. KELLOGG: It seems to me if you go down and look over the wares of any corner drug store you would have to support Dr. Allin that there is quite a gap between what the medical scientists know

and what the people know. (Laughter.)

DR. TAEUSCH: I should like to ask a question along that line, Mr. Osborn. These questions all seem to lie in the direction of what we are going to do with this knowledge when we get it. I should like to ask where we can get that knowledge to begin with. You mentioned three conditions requisite for progress in the science of man:* 1st, time; 2d, support; 3d, freedom. I should like to know where we have that freedom of research that would enable us to have a science of man. I am rather suspicious of universities. There is another source in these independent institutions, such as the Brookings Institution, and I am wondering what about the Department of Agriculture. How do you feel? Do you think we have enough freedom as a governmental agency to contribute anything toward a science of man?

MR. OSBORN: The objective of the Department of Agriculture, as I understand it, is primarily agriculture, and it is only recently that people generally have recognized that agriculture also has something to do with human beings.

DR. TAEUSCH: Well, is it <u>also</u> or is it <u>mainly</u> that?

MR. OSBORN: No, I think really it is <u>also</u>, because the responsibility for deciding on scientific research about human beings is mainly the responsibility of other people, but there are things in connection with human beings that you must know in order to do your work. I don't think that in my slight experience, the Department has been handicapped. Population studies are perhaps more related to agriculture than any other kind of human studies, and I think that several men in your Department have made very large and very free contribution to population studies.

DR. TAEUSCH: Well, let's take these other people for a moment. What class of institutions are free from economic and moral prohibitions against their determining the actual science of man?

MR. OSBORN: Personally, I think they are all pretty free, except so far as they are individually conditioned; but that conditioning is often the reverse of what you would expect, some people being conditioned by a certain set of circumstances to conform, and others by a certain set of circumstances not to conform. I think the best proof that there is great freedom is the violence of the controversy. I suppose there is no subject in which the emotional bias is more important than the relative contribution of heredity and environment to individual differences. It is a field of great controversy. George Stoddard and his associates at the University of Iowa, making studies

* Page 138.

of orphans or pre-school children, claim that you can take children of feeble-minded parents and make them as bright as the average. That is an extreme position. Leahy and others at the University of Minnesota, studying foster children, claim that heredity contributes from 60 to 70 percent of individual differences, regardless of environment. Such controversies indicate to me that there is almost complete freedom of approach to research in these problems.

DR. TAEUSCH: I should like to come back to Mr. Allin's question on research. His point of view is that we have to educate people in regard to what we have found out. I am wondering if we can allow research to rest with certain very definite institutions, which individually are more or less hamstrung in the publication of what they actually do find. Take certain facts that are discovered to the satisfaction of the scientists, but which interfere with certain economic or industrial interests, and you will find that it is usually very becoming to that institution not to go ahead with the dissemination of that information, or even with further research in that line.

MR. OSBORN: I never have been one of those that have felt that way and I have seen a great deal of it from the other side of the fence, because until I got into scientific work about ten years ago I was engaged in business. I used to hear then, and I hear now, a great deal about inventors having their patents and ideas bought so as to prevent their getting out into the market.* That is an example of the kind of thing you had in mind, I judge.

DR. TAEUSCH: Yes.

MR. OSBORN: I was in the business of managing inventors for fifteen years. They are delightful people, but frequently overestimate the commercial value of their inventions. I assure you that in the fifteen years during which I was dealing with small and large corporations I never saw the executive staff or the executive heads of any corporation, large or small, who weren't frantic to get hold of a new invention, scrap anything they had, get the jump on their competitors by putting these inventions into operation. The difficulties in getting a new invention accepted, if it was good, always lay with the technical staff, the research staff, whose general attitude was that if the invention was any good they would have thought of it themselves. That was the human reaction. Now, among the inventions that I handled there were three that were good. We looked at approximately ten thousand in this ten-year period, and my best guess was that while of course we missed a

* Cf. Dr. Sigerist's remarks in this connection.

good many tricks in picking the three good ones that we spent most of our time on, 90 percent of the inventions, although the inventors themselves thought they were revolutionary, were not really practical things; and therefore 90 percent of the inventors thought that a selfish and unkind society was preventing the acceptance of their inventions.

My feeling about the throttling of other kinds of research by vested interests is the same. Possibly my experience with inventors makes me a little recalcitrant there. I have had some experience with various foundations of this country. I never heard the least indication that because a piece of research might upset an established precedent, an established economic institution, that the foundation should not back it. I should say that in my own experience, the boards of the large money-giving foundations were more experimentally minded, more willing to take chances than most people. Now, that is just my personal experience.

MR. TOLLEY: Mr. Osborn, might I see if I understand. I think you are saying that the lack of progress that we fear, the progress that you hope for but fear will not come, in this science, will be due more to the research men themselves than to society at large.

MR. OSBORN: No, my fear that there may not be progress is based on a number of present conditions. One is that there is no particular reason why anybody should be doing research on these problems. There isn't the economic drive behind research in human genetics, for instance, that there is behind research on the genetics of cows. There is a drive on the part of the psychologists who are studying the results of education, but the support of those psychologists has to come from more or less independent sources. Many psychologists, both radical and conservative, are supported by foundations, and their funds, I presume, in the next ten or fifteen years will be more and more limited. I am not arguing for or against the limitation of their funds, but simply recognizing the probability that large fortunes will not be available for such purposes in the future.*

DR. ALLIN: Where are we going to look for the people who shall open up our science, aside from institutions we already know?

DR. TAEUSCH: What is going to be the real future of the science of man from the research angle as well as the dissemination of knowledge?

MR. OSBORN: I think it will gradually go into the universities and institutions and into our government agencies, just as other things which have been started independently have gone into it, now that the science of man is already largely in the universities, in many cases supported by outside money because it is a new thing. There was no science of population fifteen years ago and there was no population taught in any of the universities. It started in three independent ways: 1st, with men who became interested in it on the side from their regular work;

*Cf. Professor Carlson's lecture.

2d, with the Millbank Fund as an adjunct to their work in public health; 3d, by a man who had made a fortune in the newspaper business and thought here is an interesting thing that ought to be done, and so set up the Scripps foundation for population research. Some work was also done at the American Museum of Natural History. That was the condition twelve years ago. Today Princeton University has a chair of population. The Graduate School of American University has a chair in population. So has Howard University. A couple of other universities are considering having chairs of population. Thus population is going into the regular university's job of teaching, with research on the side. That has already been done in psychology to a large extent. I hope and believe that human genetics is going into places like the great universities and medical schools in the next five or ten years. I believe it is on its way. Those institutions that are now supported by private funds I think are going to be supported by government funds.

DR. DEMING: What would be your plan of educating the public, putting such studies into the colleges or high schools and so forth?

MR. OSBORN: I don't think that the science of man is sufficiently developed to justify trying to take it out to the public very much, because I think it is a great mistake to educate the public in the elementary stages of any science. They get a lot of misconceptions that are hard to eradicate. I think it is a mistake for Stoddard to be popularizing his work so much. I have a great respect for him, but it is quite possible that ten years from now his views will be different from what they are now. Carl Brigham once came into my office and saw a book entitled "A Study of American Intelligence" by Carl Brigham. He said, "Have you been reading that? Write across the title-page that all conclusions in this book are hereby certified to be incorrect. Signed, Carl Brigham."

DR. DEMING: Is there a question whether the <u>scientific method</u> can be entrusted to the public? If you are going to wait until you have facts to entrust to the public, you never make anything public; but is there any harm in letting the public know the scientific method and that conclusions do change? Science is a changing thing, self-correcting.* Can the public be educated up to that?

MR. OSBORN: I think that the public should be informed of all scientific knowledge about at the point where there is pretty general agreement on that knowledge by competent authorities, but I don't think it should be informed of scientific knowledge when one individual has come out with some startling new discovery that may not be confirmed by other scientists after him.**

* Cf. Professor Cohen's lecture. ** See Dr. Lorimer's remarks further on, p. 158.

DR. KELLOGG: I rather get the impression, perhaps wrongly, that you are talking of sort of an aristocracy of science now, sort of a closed corporation, and that when we settle the situation among ourselves, then the aristocracy agrees that the masses are ready for it. That puts it pretty crudely, but that is what I got from your idea; you would object to scientists popularizing things until there is pretty general agreement. Rex Beach published something that caused the Department of Agriculture to answer many thousands of letters.

MR. OSBORN: I have been impressed by a great deal of the half baked science that the newspaper reporters get hold of and run headlines on, and at times it causes quite a state of confusion. I would like to see the public fed more accurate knowledge. I am only hoping that scientists along with other people will in general be modest and not be anxious to go before the public with their claims until they have some important grounds for feeling that there is real work behind their results, of a sort that other competent scientists can verify.

MR. GLICK: We were discussing what chances there were that there would be widespread social support for an increasing amount of research in the development of the science of man; also whether we may not be approaching the time when the whole climate of opinion would turn more favorably to that kind of thing. Don't you imagine that the period of wide-spread disillusionment of science itself, as to why it is that the results of science have so much more frequently been used for destruction instead of creation, may in itself be a prelude to just the same kind of social state for increasing interest in the science of man?

MR. OSBORN: That is a very helpful thought. I hope it is so. It is quite possible it may be. Perhaps it has been unconsciously in my mind in the development of my own ideas. I think if we can develop the idea of the possibilities of the science of man, and the recognition of its present limitations, we may give the public a more hopeful and a more patient attitude about the science of man.

REMARK: Isn't that necessary to prevent a repetition of what is happening in some countries where all the "recognized" scientists in the field (those who don't agree are not "recognized") have come along with a particular set of ideology which the government then proceeds to put into effect. In Germany today all the "recognized" scientists are in agreement, aren't they, with certain theories on what we call the science of man, which they call in other terms? Those that don't agree are not "recognized" scientists.

MR. OSBORN: I just don't know. Recently I have been talking to Clyde Keeler, just home from a year abroad, working on human genetics

on a Guggenheim Fellowship. In Russia ten years ago, they were doing a great deal of work in human genetics and doing it quite freely. They had some very good men. Unfortunately they didn't have so many psychologists as they did geneticists. But they had a tremendous opportunity. The government supported them with a great deal of money. It is said that when they wanted some identical twins to study from an early age, the government shipped them eighty-three pairs of identical twins and said "Go ahead; do anything you want." It was an unprecedented opportunity. They set up a number of organizations with names and addresses. Keeler went over and found the names of the organizations on the doors but no work going on. Apparently somebody had got the idea that this was a bad thing to be studying in a communistic state, and the whole thing was abandoned.

With respect to Germany, I asked him why he wasn't talking about what he saw. He said "It is embarrassing; everybody thinks I am a Nazi if I talk about what I saw on human heredity in Germany. The men in Munich have large funds available to them and apparently they are just as free in their work as they can be anywhere and they seem to be doing a good job." He said "I am afraid to talk about that because everybody said 'You have been sold on the Nazis.'" But the same thing might happen then that happened in Russia, so I find it very difficult to know how free they are.

DR. ENGLUND: Is it possible, Mr. Osborn, that the inhibitions imposed in Russia might have been due to the inclination or belief by someone in authority that this study would lead to a recognition of the individual and that therefore it was incompatible with their political ideology?

MR. OSBORN: I should think it is quite possible.

DR. ENGLUND: That leads to another question. You indicated or mentioned a moment ago that some years back you felt, as I think lots of other people did, that if you could fix the physical environment of man, he would be all right. Happiness, however we define it in relation to our standard of values, would come about. Now we are turning away from that a little bit, perhaps with some disillusionment, and more and more accepting the idea of social responsibility for the happiness of the individual. I am wondering if we are going to find the same sort of limitation in that approach that we seem to have found in the physical improvement of the environment, and whether we shall again turn back from the extreme emphasis on social responsibility and social technique to the science of man as a means of improving the happiness of the individual. There was a time in the stage of extreme individualism when the individual was somehow responsible for his situation in life, no

matter what it was; and if any improvement was to come in that situation it was up to him. Now we have come a long way--I say we in the sense of general public thinking--to the point of view that society is responsible. Almost nowhere in authoritative circles do we now hear any emphasis placed upon the individual's responsibility for what he is and might become. In fact, if you suggest such a thing, you lay yourself open to the charge that you are dragging a red herring across the trail of social responsibility. But suppose we pursue the approach of social responsibility for the welfare of individuals as groups; are we likely to find in that approach the same sort of disappointments that we found in fixing the physical environment by the application of the results of scientific research? Must we come back to the point where I understand you to be now, that we have got to turn to the science of man because social responsibility won't achieve happiness, unless by the science of man you can do something with the individual?

MR. OSBORN: Yes, I think so. I think that we shall ultimately find that social conditioning is limited by genetic factors, and that there are genetic limitations to happiness as well as social limitations to happiness, and that with the recognition of genetic differences we shall go back to a fuller recognition of individuality and personality as something flowing from within. In other words, I myself believe that the recognition of genetic factors in individual differences is a recognition of the individual values of man apart from his socially made values, and lends more to the dignity of human beings than the theory that we are all alike by heredity.

DR. CHAPLINE: Is it possible that security may be a big factor in happiness?

MR. OSBORN: I can't answer your question as to happiness being related to security. I would feel as you do in theory, and yet I am told by friends who go out to China that the Chinese peasants or coolies are the happiest people they have ever seen anywhere, yet they certainly are the least secure. I know wealthy people who economically are 100 percent secure as to position, and it seems to me the more secure they are the more disturbed they are about conditions. So in practice I just don't know where we stand.

MR. WILSON: Mr. Osborn, you closed yesterday on that note of the problem of values and their connection with religion. If you take the point of view of culture, the anthropologist looking at any society (and we are in society just like the people in Polynesia; we have our culture and so forth), aren't those things really the directing forces that determine what we shall do with science and with the science of man? In the end,

won't it depend on what we do with religion and culture?

DR. LORIMER: Mr. Osborn, to go back to the matter of education, I should like to ask if there isn't a distinction in your mind between what you might call the normal channels of education, the dissemination of ideas through real education, and what might be called the broadcasting of findings; that you would be very glad to have immature science, undeveloped and conflicting, taught in universities and studied by the students who are participating in the creation of science. This is along the line of what Mr. Deming said,* that the development of teaching of the scientific method wasn't the thing you were protesting against, but the broadcasting without this opportunity for examination and critical consideration; the broadcasting, as final, results that are really immature. I mean you were not advocating any sealing or locking up of information, but rather that you are in favor of attempting to spread undeveloped information through the normal processes of education, where there is opportunity for real examination--all this in contrast to rushing to the radio with it.

MR. OSBORN: Exactly.

MR. WILSON: Well, we have had a very interesting session and we want to thank you, Mr. Osborn, for coming down and speaking to us yesterday and participating in this seminar with us today.

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* Page 154.

Ten Lectures and Discussions on

SCIENCE: Its history, Philosophy,

and Relation to Democracy

held at the Department of
Agriculture Graduate School
under the chairmanship of

M. L. Wilson
Under Secretary of Agriculture

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Edited by W. Edwards Deming
Assistant Director of the Graduate School

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Lecture VII

SCIENCE AND DISCOURSE

by

Charles W. Morris

Associate Professor of Philosophy
The University of Chicago

The Graduate School
The Department of Agriculture
Washington
1939

Lecture VII

SCIENCE AND DISCOURSE

by Charles W. Morris
Associate Professor of Philosophy, The University of Chicago

I

As you are all aware, we are living in an age that is peculiarly conscious of its language; we have today a linguistic consciousness of a very striking order. This has been so in certain previous periods of history, as in the Hellenistic period, and in the late Middle Ages. Our language is not working very well, and we have become conscious of it as an instrument. The market is flooded with books that offer various linguistic panaceas for our scientific and social ills.

Language is an enormously important instrument, and so far as our ills are linguistic ills, then linguistic analysis is important; but I think we should have a certain skepticism about the utopian mentality in the domain of linguistic analysis. Nevertheless, with that reservation, I am going to make a linguistic approach today, for a very specific reason. I am going to try to deal with science in terms of scientific discourse, that is, by a consideration of some features of the language of science; and I do that because, in the sense in which we are now using language at this moment, there is no animal other than man that has language in that sense of the term. Man is the linguistic animal par excellence. It is natural therefore that all human activities develop various appropriate forms of language. We speak of various types of discourse, such as religious discourse, mathematical discourse, esthetic discourse, literary discourse, and so on; and scientific discourse may be approached in terms of its relation to other types. In this way we get a rather novel approach to the relation of science to other human activities.

Since I take it that the consideration of this relation is a part of the program of these lectures, I shall try to characterize the nature of science by a consideration of scientific discourse, and by contrasting it to two other forms of discourse which I shall call the <u>esthetic</u> and <u>technological</u>. From my point of view there are only three primary forms of discourse; all the other modes of speech that men use seem to be a function of these three primary forms. Consequently, the question of what science is, or what scientific ideas are, shall be reformulated as the question "What terms and sentences appear in the language of science?" When we raise the question of what "scientific ideas" are, it might seem as though we are making psychological investigation concerning what occurs in the mind of the scientist. Naturally, the scientist has many ideas that we could not call scientific ideas, and we are in a rather nebulous sort of realm; whereas if we ask what terms and sentences occur in the language of science, we have a certain concreteness in our approach.

II

I shall take as a simple example of scientific discourse the following sentence: "The body x has a mass of 3 grams and the body y has a mass of 6 grams; hence y has double the mass of x, and if the same force acts on the two bodies, the acceleration of x will be double that of y."

If we analyze this simple bit of discourse as characteristic of scientific language, we may call attention immediately to four aspects of it, which, taken together, characterize fairly well the nature of scientific language and, indirectly, of science.

The first thing that we note is that the signs in the discourse take on the form of statements; that is, this discourse has the character of statements. Commands are not found in it, nor terms that would be expressive of emotions or feelings of the scientist. The character of the discourse is that of statements which state that certain characteristics or properties are found at certain space-time points. That is, the body x, which would be a body inhabiting a certain region of space and time, has a certain character, a mass of 3 grams, for example; and a prediction is made that if a certain force acts upon these two bodies, then the acceleration of x will be double that of y. Therefore these statements are of a peculiar sort; they are what we may call predictions that ascribe certain properties to space-time regions, so that if something is done at a certain space-time point or region, such and such a result is predicted. It seems to me that this character of prediction, and the accuracy attained in prediction, is the basic characteristic of science, determining all its procedures and its form of discourse. As I see it, the goal of science which distinguishes it from other human activities, is accuracy in prediction, and the reason why accuracy in prediction is important is because men acting at one point in space and time wish to prepare themselves as well as they can for what will occur at other points in space and time which are now remote. That is, they want to be able to determine what they have to take account of, what they are going to encounter, wherefore accuracy in prediction is, I think, the basic feature for which science aims. It might be said that science aims to give reliable knowledge, but I think the test of reliable knowledge is accuracy in prediction. And I believe that all of the essential characteristics of the language of science can be shown to depend upon this goal.

As an example, in its statements, science has developed various techniques for making accurate references to space and time regions. The whole development of coordinate systems, the whole development of mathematical expressions, are ways in which statements can be given this type of precision which accuracy of prediction demands.

* This reminds the editor of C. L. Lewis' statement,"... there is no knowledge of external reality without the anticipation of future experience. Even that knowledge implied by naming or the apprehension of anything presented, is implicitly predictive, ..." Mind and the World-Order (Scribners, 1929) p. 195.

The second characteristic of scientific discourse we may call confirmability. Scientific sentences are anchored in observation. That is, we have to be able to determine whether the body x does weigh 3 grams, and whether the body y does weigh 6 grams. We have to be able to determine whether one acceleration is double another acceleration. That is the kind of thing Bridgman means in stressing the operational type of procedure. We have to have some type of procedure by which we can tell whether a proposed term is applicable.

The situation can be illustrated by a story that is attributed to Galileo. I do not know whether it is accurate or not. You remember when Galileo discovered the mountains on the moon, some of the Aristotleians who thought the heavenly bodies were perfect spheres suggested there might be some crystalline substance which filled in the valleys between the mountains so that the actual surface of the moon was spherical. And Galileo answered, or is supposed to have answered, "That is such a fine hypothesis that I shall assume that there are mountains on the moon made of this crystalline substance, which themselves are ten miles high." In other words, the introduction of the term "crystalline substance," if not subject to some operational test, is irrelevant in science. Galileo was therefore pointing out the operational test, or the test of confirmability, for the introduction of meaningful scientific terms.

We might think that that is all there is to the question of scientific terms: that a scientific term is one that is governed by some operation such that we can, by observation, tell whether an object has or has not the property in question; but if we look a little further at our sample of discourse, we see that that would be too simple a statement. We see, for instance, in drawing the conclusion that if the same force acts on the two bodies the acceleration of one will be double that of the other, that the deduction is made by the help of what we would call a scientific law, that force is mass times acceleration. The status of such a theory or law is somewhat difficult to determine, because there is a certain conventional element in this law, depending on the type of units we use. Nevertheless, this law is in some sense empirical in origin, in some sense it rests on observation; but here I call your attention to an interesting fact about it, the way in which it is used. It is used to allow a certain conclusion to be drawn, a certain prediction to be made, on the basis of the first two statements that the body x has a mass of 3 grams and the body y has a mass of 6 grams. In other words, we must distinguish the theoretical superstructure of science from what we might call singular statements in science, and regard the laws of science as part of the superstructure which functioned as a way of passing from singular statement to singular statements. The singular statements in science would be the sentences that ascribe certain properties to certain space-time regions. In passing from certain of those statements to new singular statements, namely, that these bodies will have an acceleration of such and such a sort, we make use of scientific theories, that is, of sentences in the scientific superstructure.

III

The question arises whether terms in the scientific superstructure have to be empirical and confirmable in the same sense in which the terms in the singular statements are; and there I think the answer is no. I think that the only thing we can demand of the scientific superstructure is that it be such that from singular sentences we can deduce new singular sentences, that is, that we can make predictions. And therefore it is not essential that terms like "atom" and "electron," or thousands of other terms in science, be given direct operational meaning at the level of singular statements; the basic point about the superstructure is determined by the test of prediction, namely, the superstructure must be such that given some singular statements it must be possible to make predictions that can be confirmed. I beleive the final and sole test of scientific theories is whether or not they allow us to make predictions. Science, in other words, admits any sentence which is a prediction or from which predictions can be deduced.

Even then we have not exhausted the terms in this simple bit of discourse, because we have there a statement that the body y is double the mass of x, because x has a mass of 3 grams and y has a mass of 6 grams. Here we are using a statement of an entirely different kind, we are using the statement that two times three equals six. A statement of that sort found in logic or mathematics, is of course different from either the singular statements or the theories by which we deduce the singular statements.

The best contemporary opinion on the nature of logic or mathematics regards such formal sentences as part of the linguistic apparatus for making deductions. That is, the use of the law "two times three equals six" is to allow us to make the specific predictions that we make about the accelerations. That is, we use the singular statements to start with, we have theories from which we can deduce predictions, and then we have the tools of deduction (logic and mathematics), which allow us to deduce new singular sentences from the theories and the initial singular statements. It was Newton, it seems to me, who first saw that mathematics is a tool of science, that mathematics does not itself give knowledge of the world but is part of the way in which scientific knowledge is organized and in which scientific deductions are made.

Such, then, are the basic levels and characteristics of scientific terms and sentences. Scientific discourse is composed of singular statements or the terms that occur in them, theories from which we are able to make new predictions, and the tools of logic and mathematics which we use in making deductions. Even the statement that a scientific system must be non-contradictory can be perhaps traced back to the single test of prediction which I am setting up as the determiner of scientific discourse, because it turns out that if a system has two contradictory propositions within it, that anything can be deduced from it. In other words, a contradictory system allows anything to be deduced, so even the test of

whether a system is contradictory seems to me to be controlled by the scientific demand for accuracy of prediction.

The last point I shall mention about this sample of scientific discourse is what we might call its intersubjectivity. I have stressed three aspects of scientific discourse; its statement character, its confirmability, and its systematic character. And now I mention intersubjectivity. There is no reference made in the statement of science to specific observers. No one says who must confirm the statements. The reference is not to observers, but to certain properties in interrelation; and I think that scientific objectivity either is or involves intersubjectivity as an essential feature; that is, it is essential to scientific discourse that the meanings of the terms be intersubjective and that the sentences be capable of confirmation by anyone who participates in the scientific enterprise.

If we were to sum up what I have said so far, it would be something like this: scientific discourse or language is a specilization of our common language for the purpose of obtaining accuracy in prediction, and the demand for accuracy in prediction controls the characteristic features of that language. The scientific habit of mind would be the habit of mind of accepting statements of the type which are admitted into scientific discourse. And scientific procedures would be those procedures that are used in getting such statements. So, while we start from a linguistic basis, we can characterize the scientific habit of mind or scientific procedure in terms of this type of discourse.

We might compare science to a building. A building is anchored to the earth at certain places, but there is a large superstructure which is not in direct connection with the earth; the superstructure however must be connected at every place with the foundation, or with elements themselves connected with the foundation. There could not be an isolated superstructure; that is the analogy I would give with reference to science. Singular sentences constitute the base; the theories and the logical and mathematical devices constitute the superstructure; the terms in that superstructure do not rest in every case upon the earth of observation, but they are so connected with the singular statements that they permit deductions to be made so that further singular statements subject to observational control can be obtained.

We can also carry out the analogy by comparison to a map. There are many features of a map that are not found in the territory that is mapped. We may use colors, we may use certain numbers, we may mark points of interest by certain signs, and while there is a certain relation between those and the territory mapped, the map is not in any sense a literal representation of all the features of what is mapped. The map, in other words, has its superstructure. Now the map must be such that if we tie it to the earth at one point--that is, if we put a pin in it and say "Here we are now"--then the map must be such that it furnishes us the means to get where we want to go. That is, it must give us accu-

rate predictions with reference to what we shall encounter at some other place in the space-time region that is mapped; but the only limitation of the map, I think, is that it allows us this accuracy in prediction, this accuracy in the guidance of our action; and I therefore would compare the scientific edifice to a map, giving great flexibility and freedom to the superstructure, provided always it is anchored to the superstructure, i.e., to the singular statements in the way indicated.

IV

This comparison of science to a map raises the question of how far science is a unity. Is science one map or many maps? And this question will allow me in passing to say something about the unity of science movement. It is interesting that throughout the world there has been growing a movement that we might call the science of science. The science of science brings science itself into the focus of scientific investigation. It becomes immediately clear that the science of science would have at least three important aspects or parts; the history of science, the sociology of science (using that term in a very wide sense, as the study of relation of science to all other social processes), and the logic of science.

The unity of science would be a particular thesis within the science of science, the thesis that science in some sense, with respect to its methods and its terms and its laws, constitutes some kind of a single structure or single map. The thesis of the disunity of science would be a thesis within the field of the science of science.

Time does not permit me to trace the history of this contemporary unity of science movement. It stems most directly from groups of scientists in various countries: Mach in Austria; Poincaré in France; Charles Peirce in this country; Russell in England; Helmholtz in Germany, and so on. The followers and students of these men became interested in problems in the logic of science, or the "philosophy" of science in some vague sense of the term, and so the movement grew out of scientific interests and activities themselves. Little groups grew up in various countries that gradually became aware of their common interests. In 1934 it was decided to hold a conference in Prague to bring together representatives of these various groups, and at that time was formed an organization which is known as the international Congresses for the Unity of Science, and which has an international committee of scientists and logicians to direct it. The first of the Congresses was held in Paris in 1935, and Congresses have been held yearly since: at Copenhagen in 1936; Paris in 1937; Cambridge (England) in 1938; and in September of this year, from the third to the ninth, the first of the Congresses for the Unity of Sciences in America will be held at Harvard University.

This group has as its main journal The Journal of Unified Science (formerly Erkenntnis) and the history of the movement will be found there. Also plans have just been completed for the publication of a Library of Unified Science, which will bring together monographs and books in this field. Most important of all is the Encyclopedia of Unified Science, which is now being published by the University of Chicago Press. The first two volumes, called Foundations of the Unity of Science, are composed of a series of twenty monographs, of which seven are published or will be published in a few weeks. Plans have already been laid for the next six volumes (sixty monographs), which will constitute a section on the procedures of science, and will without doubt be the most extensive presentation of the methodology of science in existence.

In the unity of science movement a distinction is made between three aspects of the unity of science. There is first a question of the interrelation of the methods of the various sciences. In other words, does science constitute a unity methodologically? Of course there are differences between the methods in different sciences, but by and large, can scientific method be characterized as one?

Then there is the problem of the unity of terms; namely, is it possible to construct a single structure, a single language, in which the terms of all the sciences will appear in one systematic whole? By "all the sciences" I mean not merely the natural sciences, but also the field of the social and humanistic studies--even, for instance, esthetics. Can, for example, the terms within esthetics be brought into a single logical sturcture with the terms in physics? The most extreme proponents of the unity of science would maintain that all of the sciences constitute a single unified whole with respect to terms, such that on the basis of a few terms it is possible to characterize the meaning of all the other terms in the scientific edifice, including the social sciences and the humanistic studies as well as the biological and physical sciences.

The problem of the unity of law would be the third problem. Is it possible in theory (and of course the problem of practice is difficult) to integrate the statements of the various sciences so that from a certain number of statements which may have come from one science, or possibly from a number of sciences--is it possible to deduce the theorems of all the other sciences, so that there would be a single deductive scheme embracing all the sciences? The general opinion is that no one knows whether this can be done, but that there is no known reason why it cannot be done--that is, there is no logical reason why a unity of law is not possible. It is obvious that we do not have anything like that today. We have no single principle for instance, from which we can deduce the behavior of the schizophrenic and falling bodies. We do not have any method of connecting these diverse bodies of facts, but the thesis with regard to law would hold that that type of integration is not logically impossible.

I shall not characterize this movement any further, since I wanted to refer to it only in psssing as a matter of information; but if science really does constitute a map, then while it is useful to have different sections of the country, it is possible that science in its totality be a single map.

Before passing on, I might make that possibility a bit plausible by using an illustration that Otto Neurath likes to use: suppose you notice a certain house on fire, and have the problem of predicting what will happen; will it burn down or be put out? Notice how complicated the situation is. You have all sorts of factors involved: in water you have chemical factors; you have the habits of the local fire department, and therefore sociological phenomena; you have the question whether the man deliberately set his house on fire in order to collect insurance, and therefore you have problems involving the psychology of human nature; there is a question of what will happen to the weather, so that altogether all sorts of physical sciences are involved in the picture--biological material, mathematical material, sociological material, psychological material, are all involved in such a single situation and statement.

It therefore would be very queer indeed if biology, physics, psychology, and the social studies did not present some kind of a unified aspect, because, if our world of nature is interconnected in all these respects it would seem likely that what we speak of as the different sciences must in some sense or other have a type of interconnection (a common superstructure) that develops with the interconnection of the phenomena themselves.

The very fact that in making predictions we do utilize material drawn from all the bodies of knowledge would suggest that somehow or other the terms and laws of these bodies of knowledge have a systematic interconnection. I might say that the unity of science goal should not in any sense be regarded as a panacea, or as a substitute for scientific activity of any type that goes on; I mention it as an interesting attempt of science today to look at itself as a whole and to consider its history and its social relations and its logic in the same spirit with which each individual scientist would consider his own subject matter.

V

I want now to round out this theory of science, approcahed in terms of scientific discourse, by considering briefly and inadequately two other forms of discourse, which I shall call the <u>esthetic</u> and <u>technological</u>, because such a comparison will allow us to see something of the relation of science to other human activities. Consider a little poem of Emily Dickinson called "The Cemetery":

"This quiet Dust was Gentlemen and Ladies,
And Lads and Girls,
Was laughter and ability and sighing,
And frocks and curls.

"This passive place a Summer's nimble mansion,
Where Bloom and Bees
Fulfilled their Oriental Circuit,
Then ceased like these."

Now if you gave a prose scientific transcription of this poem, and began to put in spatial and temporal coordinates, and so on, you would describe certain space-time regions, you would state that some of this physical material came from men who were of such and such an age, or biological organisms of such and such a sex; you would say that at certain times they made certain facial movements of a type called laughter, and certain movements of the respiratory system known as sighing, and so on; that once there was a home there, and that flowers and bees were about, and that these went through certain cyclical processes and passed out of existence in a certain space and time as the young male and female organisms did. What would you have lost in this scientific statement if you so reduced it to a scientific account? Well, you would have lost all the poetry. But then the question is, What is the poetry? I think it is clear what you would have lost is the presentation of the significance, the value, of these happenings. You would have described the happenings but you would not have presented the significance which they had for some person--at least for the poet. In other words, in esthetic discourse--and I think this holds for music or poetry or for any other form of esthetic discourse--you present what we might call value-properties of the thing. (I shall not discuss the status of such value-properties. I think we may regard them as the characters of things so far as they satisfy or frustrate our interests.)

Now, when we present things, as R. B. Perry would say, "interest-wise," when we look at them in relation to interests rather than in the terms that science looks at them, and are able in some medium of sound or color or what not to actually embody those value-properties that were presented, then we have a work of art, we have esthetic discourse. This poem actually embodies something of the sense of beloved things which are transient, passing away. It is not that you merely give the temporal coordinates and the physical properties, but you capture the sense of loss that is involved in the occurences. And I think that is the general nature and function of art: not to present accurate predictions (that is the function of science), but to embody valuable experiences in some medium or other.

The importance of art then becomes clear from that same point of view, because it is through art that man preserves and communicates his significant experiences; and art has its own form of discourse relevant to performing that function just as science has developed its form of discourse relevant to performing its function.

VI

Let us contrast very briefly these two forms of discourse with another form of discourse which we may call technological discourse. I take these words from a treatise on orchestration:

> "Flute solos are best soft and with a thin accompaniment. The two lower registers are good for background work but the highest is too shrill for this purpose. Flute tone quickly becomes monotonous to hear, so restraint should be exercised with regard to its use. A flute must not be expected to play softly high up in its compass."

Now the characteristic of such a form of discourse is the appearance of such terms as <u>must</u> and <u>should</u> and <u>should not</u>. All moral terms, of course (<u>don't do this</u>, or <u>do this</u>, or <u>this should not be done</u>), also fall within technological discourse. Now, what is the characteristic of such technological discourse? I think that if you take any statement, say the statement "You should have your child vaccinated," which would be a statement in technological discourse, you will find under analysis that there are three factors in it. There is some reference to a goal or end, namely, in this case the health of the child. It is taken for granted that the parent wants to maintain and preserve the health of the child. Second, there is an implicit claim that such and such a procedure, namely, vaccination, is the best method available for realizing that end. Certainly, if a physician made that statement, we would all assume that he takes vaccination to be the best procedure that is available for realizing that end. Third, there is an attempt to incite action to adopt this procedure. That is, there is the element of wishing to so influence the person spoken to that he will act in such and such a way, that he will actually go ahead and have the child vaccinated. I think technological discourse, then, goes beyond scientific and esthetic discourse in the fact that it endeavors to incite behavior appropriate to realizing such and such a goal. The aim of the statement is to get the person addressed to allow vaccination to take place.

If we conceive of morality as a form of technology--and I think that is the way to approach morality--sentences of moral discourse show themselves to be sentences in technological discourse. The question becomes: "What is the relation of morality to other types of technological discourse; for instance, to engineering discourse, agricultural discourse, and so on?" I think it is possible to regard morality as concerned with those techniques which are believed to give the maximum of value in situations where there are value conflicts, that is, where there is a conflict between things that men cherish. The concern of morality, it seems to me, is always to maximize value in such conflicting and competing situations. If time permitted I would try to bring the whole sphere of morality under technological discourse.

VII

With this as a background, I wish to spend the time that remains in connecting this conception of science with the humanistic interests, with which this course of lectures is also concerned. I want now to say something dealing with the cultural importance of science and the unity of science movement when conceived from this point of view. The preceding material gives, I think, the necessary basis for such a consideration.

The first question I shall raise concerns the relation of science to value. It is a common position that science has nothing to do with determining values, but only gives us facts and means for realizing values. Scientists themselves often take this attitude. I have heard social scientists say "If you tell us where you want to go, we will assess the means which will best get you to that goal." The corollary of such a position is that science is only a tool; that the ends which men seek, the values which they have, must come elsewhere--from metaphysics, or religion, or some place else.

This view does not seem to be borne out by the analysis just made of the relation of science to technological discourse. It is true that life is more than knowledge. There are problems of choice, decisions; men must often act on principles that cannot at the time be said to be scientific;* but nevertheless I call your attention to this interesting fact, that the knowledge of values, and the knowledge of the conditions of their realizations, actually affect the values themselves. If one realizes, for instance, how difficult it is to attain a certain end which one has in mind, the realization may change the nature of the end itself, and one may lose interest to some degree in that end. I think the psychoanalytical theory that all desires and interests remain in their full intensity, regardless of whether they are satisfied or not, is not a true analysis. The knowledge of values and of the conditions under which ends may be realized or attained, actually changes in some cases the ends themselves. In other words, I would defend the thesis that there is no body of wisdom for the guidance of mankind which is or can remain untouched by scientific knowledge unless the development of science itself is stopped. The development of science is not merely a means toward the realization of certain values, but actually influences the course of values themselves, the things that men prize.

Furthermore, let me point out that in our common wisdom (the material that some people think can guide us and is in some sense superior to science), it is often dubious whether the techniques do realize the ends they pretend to realize. If, for instance, I say that "Procedure A realizes end B," that is a statement in scientific discourse, and whether procedure A does realize that end, and whether it is the best way of realizing it, is something which only scientific analysis can determine. Let me put it in another way. The rational control which we have of technological discourse, that is, the discourse which aims to incite us to action, is ultimately found only in scientific knowledge. Consequently, it is not

*Cf. p.109 of Professor Cohen's seminar. Ed.

merely that our values themselves are affected by scientific knowledge, but the only control over our means for realizing the values that we have is by scientific knowledge, and I see no body of wisdom that can guide mankind which is independent of the increase of scientific knowledge.

That would lead to a second point. If it were true that science is important not merely as a means and not merely as a control over our techniques, but important in influencing values themselves, then, of course, the educational importance of science and of the unity of science becomes very great. There is a tendency in some recent educational proposals to minimize the wider cultural importance of science; and I do think that science must present a more integrated and unified front if it is to have the educational significance that it should have. I call attention to the unity of science movement and the possibility in colleges and universities of certain courses dealing with the logic and history and cultural relationships of science as one way in which science can continue to maintain the educational importance which it is culturally entitled to.

VIII

The last and final point centers upon a problem which is connected with contemporary civilization. We find today a struggle between various types of minds for the future leadership of mankind, and the question science has to face and is in fact facing, is whether it shall be merely a tool in the hands of some non-scientific mentality, or whether it shall be the basic and essential factor in the new mentality. I do not mean in arguing that science should perform this larger role, that science alone is enough. We need the artist and moralist and the engineer. I have tried to point out that scientific knowledge gives us the material for forming rational decisions; it does not take the place of those decisions, but it does give us the material for forming rational decisions as to what we shall prize, and it does give us the only rational basis for improving the techniques to obtain and hold what we do prize. Looked at in this way, there can be no conflict between science and human values. Science, art, and technology prove to be mutually supporting factors in a rounded personality or civilization.

I will only add in conclusion my conviction that the fate of democracy and the scientific habit of mind are closely intertwined. Democracy, as Dewey pointed out long ago, is a moral conception and not a static body of political or economic doctrines or institutions. It aims to develop a society in which each person can become a self, making his contribution to the life of the society and being sustained in his development by the society. Democracy is, then, from this point of view, not a theory to be regarded as true or false, but a way of life to be accepted or rejected. The literature of democracy is technological discourse and not scientific discourse. The acceptance of democracy involves the acceptance of whatever techniques further the moral process that is democracy, that is, the process of maximizing the value of the intertwined lives of individuals. It is an attitude incompatible with dogmatism, which is essentially the acceptance of ends and techniques withdrawn from the influence and control of scientific knowl-

edge. If the end of democracy is a moral end, then the compatible intellectual orientation is that of the scientific attitude. The years just ahead will show whether this end and attitude really permeate deeply the American people. It is an encouraging fact that a great department of the government has realized and seen fit to stress the union of the moral ideals of democracy and the scientific habit of mind, which together constitute the heritage and the promise of the American people.

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SEMINAR

following the lecture by
Dr. Charles W. Morris

Office of the Under Secretary

Syllabus

1. Summary of the lecture. Three kinds of terms and sentences. P. 174.

2. The function of logic and mathematics. Logic and mathematics do not make statements about existence; they are sciences of symbols and do not compete with the observational sciences. P. 175.

3. The ultimate goal of unification is not necessarily simplification, but rather to get a picture of the actual situation. The goals are many. One is the facilitation of communication through the whole range of science. The systematic character of science is inevitable. It is not so with other activities--poetry for example; pp. 175-176.

4. Science is not a collection of propositions, but is a <u>system</u> of propositions. The limit of systemization in science is a unified linguistic structure; this is so because prediction, which is the ultimate aim of scientific knowledge, involves so many different aspects of nature, one of them often being man himself. P. 176.

5. Some discussion of terms; pp. 176-178. A word on physicalism; p. 178.

6. Fads in pseudo-science; pp. 179-181.

7. More discussion on mathematics and physics; pp. 181-186.

8. There is an esthetic side of science; pp. 187-188.

9. Part of our work in the Department of Agriculture is scientific, and part is technological. Technological discourse should be controlled by science. Pp. 189.

10. What constitutes confirmation of prediction (i.e. of a theory)? How does a scientist reconcile confirmation with errors of observation? Pp. 190-191.

11. Human destiny need not be in the hands of scientists, but we should demand that all technological discourse be cleansed in the light of scientific knowledge. Science must invade every field

of decision and technique. We must not allow science to become subordinated, to be consulted only when convenient. Pp. 192-193.

12. From science alone you will never deduce logically what ought to be done. But with the bringing of scientific knowledge into the process of making decisions, men's values take on a form or direction that they would not take on without that knowledge. Values are not uniquely determined by knowledge. We must avoid an emotional attitude of venerating science. Pp. 194-195.

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Proceedings of the seminar

DR. DEMING: In order to get started this morning, I have asked Dr. Morris if he would be good enough to give us a brief summary of the most important points of his thesis yesterday. Dr. Morris.

DR. MORRIS: An essential point was an attempt to approach certain human activities, including science, through a study of the linguistic media that they utilize for their purposes. It seems to me that in this way we have a concrete way of going about the study of such things as the nature of science and art and technology, and their relations to each other. Yesterday I tried to characterize something of the language of science, in an attempt to orient the whole control of that language around what I took to be the function of science, namely, to enable us to make accurate predictions. It seems to me that the whole linguistic tool is trimmed for the accomplishment of that single and essential purpose.

Then I tried to say that art, on the other hand, has as its function the development of a language for the communication of significant experiences; and that technological discourse has as its function the incitement to action, the performance of certain techniques that are urged toward the accomplishment of certain ends. Morality, as I see it, is a subspecies of technology--the most general aspect of human technology.

That was the larger intellectual framework. In running over it, I tried to distinguish in the language of science three kinds of terms and sentences: those which occur closest to the level of observation are called singular statements; they ascribe properties to space-time regions. Then there is what I call the scientific superstructure, which falls into two essential parts, theories and the logical-mathematical devices. The function of theories, as I see it, is to give us ways of getting predictions from certain singular statements; that is, given certain singular statements, we have certain general formulas in terms of which we can make predictions in the form of new singular statements. It seems to me that this is the function of the theoretical superstruc-

ture of science, and that we should not limit that superstructure as some empiricists and positivists are inclined to do. There are levels of terms in the language of science, and all terms do not immediately relate to data of observation. The primary function of terms in the theoretical portion of the superstructure is to be so related to terms in the substructure that from singular statements we can obtain new singular statements; it seems to me that this is the essential basis for the control of the theoretical side of the superstructure.

The function of logic and mathematics, on this view, is not to make statements about existence at all. If you want to regard them as sciences of anything, I think they are sciences of the formal relations of language, that is, sciences about symbols, and they therefore do not compete with the observational sciences.* They are not about nature at all in any sense. They are devices by which from singular sentences, with the aid of theories, we can deduce new singular sentences. And so the function of mathematics and logic, as I see it, is part of the procedure of science, like telescopes and microscopes; they are not themselves knowledge of nature. If they are knowledge of anything, they are knowledge of certain formal relations between signs.

DR. McCALL: As a lifelong disciple of the applied sciences, I should like to hear you discuss the ultimate goal of unification, and particularly whether such unification necessarily means simplification.

DR. MORRIS: I don't think it necessarily means simplification, though I am inclined to think that that is the goal actually involved in the procedure. When we develop a language from so many different points of view, as we do in the various sciences, we undoubtedly obtain an unnecessarily complicated language, and I should expect that there would be the possibility of great simplification. In fact, it seems to me that in some sense that is necessary. One of the purposes, one of the practical results of the consideration of science as a whole is that we should get a language that will permit specialists in different fields to talk more readily together about their common problems and common concepts than they can do now.

The purposes, the goals, would be many. One is the facilitation of communication throughout the whole range of science. As I say, I think that will involve simplification, although I do not mean to assert that it must. Another purpose arises from the fact that science inevitably sweeps everything into its scope unless it is prohibited socially from doing so. It is bound to sweep itself into its scope, and the scientific study of science seems just the inevitable extension of the scientific development itself. All sciences are involved in the science of science, or what we might call "metascience." So far as systemization goes, it seems to me to be involved in the very development of science.

* The reader may recall Professor Cohen's remarks on pp. 101-102.

I think the systematic character of science is essential. It is not so in poetry. There is no particular sense in which each poem must be conceived as forming a part within a larger poetic structure. But in making predictions, we often have to go from singular statements about some part of the world to other singular statements about another part; and as I tried to indicate, economic beings, sociological beings, chemical systems, interacting and complex sociological systems, are not separate as the sciences tend to be separate.* The result is that the making of predictions involving factors in so many fields seems to me inevitably to tend to systemization. Science is never a collection of propositions but it is a system of propositions. The limit of this development would be a unified linguistic structure with a common set of terms and common laws that would embrace the whole field of science. I do not lay any stress on the reaching of the goal. But it does seem to me that the tendency towards this goal is inevitably a part of a scientific enterpriae.

I have noticed that many scientists resent the unity of science movement in certain ways. That is a common reaction. Another common reaction among the younger men is that they become too enthusiastic about it. I think it is important to know that there is no panacea in the concern of science as a whole that will do the work of the individual scientist in his special domain and therefore it should be looked upon as an addition to scientific interests and not as a wonder-making formula of any kind. In a day like this, the tendency towards intellectual systemization is so pressing, such an emotional factor, that this kind of thing is capable of exploitation, which I think we must resist. I regard it simply as an adjunct to scientific activity in general; by no means to supplant anything that goes on, and by no means a panacea.

DR. SEEGER: Dr. Morris, I don't quite see one point. I don't see how we are going to get a common word in the different fields to cover the different meanings. As a matter of fact it is a thing I resent when I find my psychiatrist friends talking about mental energy and applying to the term mental energy all that is meant by the term energy in physics. Could you give us one example where a functional term would bridge two fields like that?

DR. MORRIS: First, let me point out that what you are asking for need not be involved. I mean that in making the sciences the object of your investigation, you would have to call attention to the many variations in usages in terms. Take iron, as used by the chemist, or metallurgist, or craftsman who is making furniture, and so forth; iron functions in many different ways, so the program is not precisely what you have indicated as necessary. On the other hand, it is true, isn't it, that if

* As Dr. C. A. Browne once remarked, in effect, nature is singularly unmindful of man's arbitrary division of nature. Ed.

the economist is talking about man, and the sociologist and the chemist are talking about man, we do want "man" in some sense or other to be an identifiable term as the object of all their investigations. They are all talking about man in some sense or other. You can simply define the term "man" as including all the attributes that the economists, chemists, and so on, find in him--but this would not be very serviceable.

There is, however, another part to the problem. Let me indicate it more concretely this way. It is a thesis which some members in the unity of science movement hold. The problem is this: is it possible to take a number of undefined terms, upon whose usage the scientists will agree (in the sense that even though they don't define the term they will use it under similar circumstances), and then from those terms be able to state the meaning of all the terms in all the sciences? If this were so, we should have a terminological unity of science without blurring the distinction between the usages of terms in the different sciences.

DR. DEMING: Is not the definition founded on the <u>use</u> of a term the only definition possible in science?

DR. MORRIS: Yes, but we have to make the distinction between giving the formulation of the rules for the usage of the term explicitly in terms of other terms for the simple reason that in any system in which you are going to define terms, there must be logically within that system some terms that are undefined. Now we actually try to select the terms that are undefined in such a way that there is a common agreement of usage, even though we cannot in the system state the meaning of those terms. (That does not mean that there are undefined terms absolutely undefinable, for undefined terms in one system may be defined terms in another system. There are no terms strictly undefinable, yet within any system there are undefined terms.) Now the thesis of physicalism is this: we can take a few terms from the language in which, in common use, we deal with what we call physical objects, like this table, or a pen, or a rock, and on the basis of those terms we then define or introduce all the terms in the physical sciences, and then all the terms in the biological sciences, and then all the terms in the psychological and social sciences, and finally even the terms in the humanistic studies--terms used in discussing psychology, art, and literature.

Now, if that thesis is sound--and I think that there is very good evidence that it is sound--it wouldn't mean that you are trying to take a term like "man" and define it by a term that would run through all these fields; but rather that you would be stressing the unity of terms in science in the sense that there is a single vocabulary of science such that from a few terms it is possible to state the meaning of every other term. As I say, there are those who oppose it

in any form, and there are those who believe that while science has that kind of unity, that unity cannot be based on the terms that one encounters in dealing with physical objects. Some think we must take all basic terms in the social sciences, and some say from psychology, whereas physicalism is a thesis that the meaning of all terms in science can be stated on the basis of a few terms taken from the language about physical things. I myself think that thesis is correct. I think it is possible actually to exhibit--and we shall attempt to do it in the International Encyclopedia of Unified Science--a hierarchy of terms in science such that there is no break anywhere in the series, on up to psychology, biology, social science, and even esthetics; that we can construct a single hierarchy of terms based on a very few, half a dozen at the most, undefined terms. I don't care to defend that thesis so much as to illustrate concretely what would be involved. That would mean, that when you got to psychology or social science, you would have to take up the basic key terms and show how they could be stated in terms of other terms at a lower level of the hierarchy of terms you are constructing. If that thesis can be carried through, then you can show the unity of the terminology of science. I think you should in the meantime keep your skepticism. I think a scientist should always be skeptical about anything. That attitude is inherent in scientific men.

DR. TAEUSCH: I should like to ask about a different type of term. You use terms referring to objects. Let's take a functional term like rationalization. There is a term which in logic and psychology has come to mean one thing, and in economics something different. Take cycle, such as you use it in business cycle; the word is carried over from physics; those words have history; language is an organic growth. I wonder how you would handle terms of that sort so as to reconcile what practically amounts almost to a contradiction in terms.

DR. MORRIS: Again we must always keep in mind that our purpose is to exhibit the actual situation; second, if we can, to make improvements in it, and keep out the panacea element completely, simply making science the object of our investigation. So far as we have different usages, we must call attention to it. Part of the clarification would be outlining the situation you mention.

DR. TAEUSCH: Are you not attempting to create an Esperanto of some sort?

DR. MORRIS: I wouldn't say that could not come or that you couldn't make some steps in that direction. There was a dictionary that a Canadian, I believe, got out some years ago. He wanted to get a dictionary all in English. That meant he had to have a certain number of terms to characterize a number of other terms. It turned out that with a relatively small number of terms, a few thousand, it was possible to characterize all the other terms of English. Now you know the scheme of Basic English, where you have a basic vocabulary of 750 terms, with which it is possible to state an enormous amount of material, even scientific material. I believe that

our linguistic apparatus has become enormously unwieldy and complex, unnecessarily so; but how much actual simplification would be obtained in that procedure we shall have to see.

If you found the use of the sign-vehicle, that is, the sound or mark which is a sign, appearing in different places as different symbols, then it isn't the same word in any significant sense; it doesn't have the same meaning. And if we wish to attain an orderly structure of terms in the sciences, it seems to me we should dispense with such terms. For instance, I have found it convenient to dispense with the term "meaning" in discussing the whole theory of signs. It is somewhat cumbersome not to use it, because we have to use whole phrases in some cases, but the term "meaning" has become so ambiguous and houses so many levels of analysis within it, that it has simply proved to be intellectually an obstruction. That does not mean that if I were talking to you individually on the street I wouldn't use the term "meaning." I would have to. We haven't a vocabulary in which we can dispense with it, and such double meanings will continue in common speech. But within science itself we can at least point out that such terms are not the same meanings.

DR. KELLOGG: I think your illustration points out a difficulty that is coming up in my mind all the time, whether in our extreme suppositions, not only in science but in all intellectual fields, we all have exactly that same experience. Now it happens that the speciality that engages my attention is relatively new, soil science. We have inherited a lot of terms from botanists and agronomists and biologists, and have had to do just like you, throw them out, not use them. So we are developing a set of new terms. However, even at the present time, if a botonist or an agronomist or an engineer, or some other specialist, is developing some thesis or some subject which requires that he go into our field, then there is great difficulty. He has a combination of the old terms that we have thrown out, without his knowing it, and he has also our terms for which he doesn't know the precise definition. So I am just wondering how you are going to do this thing without making more errors than you avoid.

DR. MORRIS: The point would not be that all the sciences would use the same terms. It seems to me what you are doing is perfectly correct. If other sciences had similar bodies of terms, a man from one group could find the relation of your meaning to his by seeing your terms in terms that would be common to both groups. Not that the chemist must use the same sounds that the biologist uses--that would tend, on the whole, to confusion. In fact, I am inclined to think that that tendency should be opposed; e. g. the use of terms analogically taken from physics, as was mentioned before--"mental energy," and now the use of a pseudo-mathematical terminology in psychology (the use of terms such as tensor in ways that are very remote from the mathematician's tensor). It seems to me that all this is apt to be intellectually very confusing, and the moral is not that the same sounds or marks should be used in

all the different sciences, but first, that there would be some common terms, as, for instance, if we are dealing with man in the different fields, we ought, somehow or other, to know whether we are talking about the same space-time being. We then ought to have a kind of verbal chart or map in which the meaning of the terms in the different fields can be so characterized that interconnections between them can be made so that one man may utilize the material of another for predictions that draw upon the material from several fields. Otherwise, if that is not really possible, then the scientific enterprise would come to an impasse, because none of the materials of one field could be used by any other worker in making predictions or computations.

DR. KELLOGG: That is what a lot of them think now.

DR. MORRIS: But if they go to the limit, the enterprise of science is ended.

DR. KELLOGG: I think your vision is a sort of huge cooperative enterprise where the basic material in the different specialities meets the specialist and moves up the scale, coming to the attention of men who develop the interrelationships.

DR. MORRIS: Yes; the first point is to find the situation as it exists. It may be that we shall come out with a recognition simply of the chaotic disunity of science and that nothing can be done about it. If that were so it would be a scientific result. The point is to make science an object of scientific investigation. So I myself stress much more the general development of the science of science than any special feature of it, such as the unity of science.

DR. KELLOGG: I think it would be an interesting enterprise, but I can see some enormous difficulties. In the more basic science, such as chemistry, you will find certain terms in agreement, such as the term "sodium," but in the newer sciences a lot of terms are just fads. Every man creates his own terms. Then another thing which I think is true though maybe it isn't: the further away you get from chemistry, physics, mathematics, into all the present-day fad-like "sciences," the more you find people to be reacting emotionally to the things they are studying, and at the same time that is the thing which confuses us.

DR. DEMING: Dr. Kellogg, if a group of people headed by Dr. Morris would achieve some unification of scientific terms, it would only leave more room for the kind of people you are talking about.

DR. MORRIS: We could ask each group of people what it is doing: are you uttering scientific or esthetic or technological discourse? You would be amazed what that simple question would do to political speeches and articles in journals which pretend to be scientific. Suppose each speech or article were analyzed to find what part is scientific, what part is esthetic, and what part is technological; that would go a long way to-

ward unmasking it. If a scientist is talking esthetically, we ought to have some way of showing he isn't talking as a scientist.

DR. KELLOGG: That is one of my personal habits, but it doesn't increase my popularity.

DR. DEMING: Now we have had some discussion on the linguistic aspects of this problem, but there are a great many other questions that came up in my mind from the lecture yesterday, and I am sure also in the minds of others.

DR. TUCKERMAN: I had several points in mind. I agree thoroughly with the distinction between singular statement and generality, but disagree with the thought that there is one right theory and no others. I am reminded of some lines by Kipling, "In the Neolithic Age":

"There are nine and sixty ways
Of constructing tribal lays
And every single one of them is right."

I feel that the basic underlying theories of science, the relationships, must be the relationships which we can verify, but the forms in which we express them whether by particles, or waves, or what not, those forms can be shifted around to suit our convenience provided they do furnish us the same relationships. Further, I wonder about the supposed distinction between mathematics and physics, mathematics being considered as something outside of and entirely different in character from physics. I understood you to say that the physical sciences, chemistry, for example, were self-corrective in that chemists asked questions of nature and found that their assumed answers were either right or wrong.

Is mathematics essentially different in this respect? Have not mathematicians throughout all history been asking questions about the ways in which people think, that is, the ways in which people find they can agree that they are thinking correctly. Have they not all these years been setting up standards and canons of thinking and then going back and correcting them in the light of experience?

I am thinking, of course, of the postulates of Euclid and the many, many years in which the scientists of the world felt the parallel postulate was an essential part of human thinking. Kant thought that and believed that the parallel postulate of Euclid was one of the a priori judgments from which the human brain could not escape. We have come to find that it is not a necessary part of human thinking. I am thinking of the way Euler used to handle his series expansions. People used to think you could expand any function in a Taylor's series. Along came Abel, Bolzano, and Weierstrass. They said "No, that isn't correct thinking, we must think otherwise" and in course of time all competent mathematicians agreed with them. So all through the history of mathematics

there has been a continual correction of the agreement as to what people will consider good reasoning. Why is it that certain mathematical reasoning is today accepted by all of us? We say, "That is good reasoning," but another man may come along tomorrow or twenty years from now and say it is not, and we may all be forced by his logic to agree with him. There is quite a bit of such disagreement in mathematics today. Isn't that on the same basis as in the case of the physical sciences? Are we not dealing here with part of this world we live in, the way human beings can agree to reason and say it is right?

DR. MORRIS: Well, as most of you know, the whole foundation of mathematics is something people have been talking about a great deal in the last twenty years. There are two major alternatives; one is whether mathematics is really the most general part of empirical science (there are those that try to maintain that thesis), and in that case you wouldn't have anything in the scientific edifice but singular statements and theories. Mathematics would be the most general type of theory, and mathematics would be in that sense about the world. This is a rather congenial attitude to the empirical scientist. He is rather inclined to think that the statement "two plus two equals four" is somehow or other an empirical statement, just like "force equals mass times acceleration." The other alternative is that mathematics is a linguistic medium.* Men may be able to state the same truths about the world through different linguistic media, just as the German can state anything that happens, the Frenchman can state anything that happens, and so on. There is no unique correlation between the French language and nature such that every feature in the French language is to be found reflected in nature. Gender in French, outside of the few cases in which it refers to sex, is only part of the formal structure of the language. It is a device for showing which words are connected in which way; it does not refer to the subject matter at all. So when you translate a French sentence about a white table, for example, une table blanche est belle, into English, you don't make any reference to gender in English; gender is not part of the descriptive content of the sentence; it is a syntactical device. And similarly some languages make a great deal out of case endings. English prepositions are used instead of Latin case endings. Prepositions and case endings are different formal devices for accomplishing the same end, but there is nothing unique that determines our choice. We can have a language like English or a language like Latin; we can say the same thing in either, but our formal structure is different.

Now that attitude has gone over not only into theory but into mathematics. You remember Poincaré felt there could be an infinite number of theories as an explanation of any given set of facts. And this attitude has led to a very interesting view today, namely, that there is nothing absolute even about our logic. There are perhaps alternative logics; logic itself may in some sense or other be optional; it may be part of the formal structure in terms of which we think about things but not itself uniquely determined by things.

* Cf. Prof. Cohen's similar statement; p. 103.

So if you put it this way, you are making your test dependent upon correct thinking. Science wants those modes of inference which will never lead us into falsity if the statements from which we start are true. That is, we don't ever want to deduce false propositions from true propositions. I have grave doubts whether by "correct thinking," from the formal point of view, we mean more than a type of intellectual process such that, given true statements, we can never thereby get false statements as deductions. Now, it may be that there are alternative procedures in that sense, alternative logics. We should all agree that if you have a true singular statement and make a logical deduction from it you will never get a false singular statement. It was of course the bringing into existence of the non-Euclidian geometry that sharpened the thinking about the subject-matter of mathematics. The position that I am representing here is that geometry in the mathematical sense is not an empirical science. We have to make the distinction between geometry as mathematics and geometry as physics. The latter (the problem of the nature of space) is a problem for the physicists.

The investigation of the alternative geometrical structures gives the investigation of what theorems we shall get if we have certain postulates. That is a mathematician's problem. From this point of view we could not regard mathematics as the most general empirical science. The various types of geometry, for example, would not from this point of view be regarded as general empirical sciences but they would be regarded as investigation of alternative linguistic structures.

I don't want to defend the latter thesis. I want to make clear that there are these two attitudes, one trying to assimilate mathematics to empirical science, and the other trying to assimilate mathematics to part of the procedure or formal structure by which we may look at the world, this procedure being perhaps suppositional and flexible within certain limits.

DR. TUCKERMAN: Suppose we take the question of the representation of electromagnetic phenomena by a field theory or their representation by means of retarded potentials. By the latter one is looking at electromagnetic phenomena in terms of action at a distance, and by the former in terms of immediate action. Those two can be shown to be equivalent in the sense that they both lead to the same results.* One is an easier way of handling the phenomena; that is the only essential by which they can be distinguished. They are two different ways of representing the phenomena, and both can equally reproduce the relationships involved. The method of retarded potentials is usually much more cumbersome than the other, though in some cases it is less so. Given these differences in the mathematical bases upon which the relationships can be reproduced I fail to see the distinction you make.

* The reader who wishes to pursue this thought may consult Alfred O'Rahilly <u>Electromagnetics, A discussion of fundamentals</u> (Longmans, 1938)

DR. MORRIS: I am not competent to take the assumption you gave and press it through, but suppose we consider the difference between Newton's second law on the one hand, and the postulates in Euclidian or non-Euclidian geometry on the other. The law connecting force and mass and acceleration (Newton's second law) is in some sense or other obtained on the basis of empirical generalizations, for instance, by the use of careful spring balances in pulling a body and noticing how the acceleration depends on the masses of the bodies. The argument there would be that in some sense the second law is based upon empirical material, and generalizing. (In generalizing, there is a difficulty; there is a question whether it should any longer be regarded as true or false, or as some persons maintain--Dewey among others--that the function of theory is purely instrumental, of going from singular statements to singular statements, and that we should not talk about a theory in its general form as if it were true or false.) Nevertheless, in this case it seems clear that the second law comes from certain types of empirical phenomena, and that it is empirical in a way that the parallel postulate is not empirical. Such would be the question.

If you merely took over force and mass and acceleration and defined the relation to be such and such, no longer regarded as open to any doubt whatsoever, simply laid that down as a definition, then of course you would be taking those signs as simply part of the structure of the language in which you are going to talk about facts. At that point the difference between physics and mathematics would become difficult to determine. But in the ordinary use of science we do make a distinction. In the example I used,* (in saying that if one body has a mass of three grams and another has a mass of six grams, therefore the second has double the mass of the first), such a statement doesn't seem to be an empirical matter at all. "Two times three equals six" is not empirical. "Double" involves that two times three equals six; the problem is not an empirical one at all. Whereas, if I say a given force would impart to one of the two bodies double the acceleration of the other, my statement is not simply something that follows from the meaning of terms, because empirically it might have turned out to be different; whereas it is necessarily true that if one body weighs three grams and another weighs six, the second does have twice the weight of the first. But I don't want to appear dogmatic. It is a crucial issue in the foundation of mathematics as to whether mathematics can be assimilated into the general pattern of empirical sciences, or whether we should have a special group of formal sciences.

DR. DEMING: I might say to Dr. Morris that when Professor Cohen was here two weeks ago, he spoke of science as being self-correcting. And that raises the question of whether mathematics is simply a tool. I think you said yesterday that mathematics is a part of the linguistic apparatus used in scientific prediction, that it is one of the tools of deduction, but as Dr. Tuckerman brought up a few minutes ago, mathematics itself is self-

* Section II commencing on p. 160.

correcting. It is self-correcting just like chemistry and physics and some of the other sciences. Euler (1707-83) did certain things in the summation of series that we should not dare to do now, even if he did obtain useful results. Our notions of limits, convergence, and summations are different now, because it was found by Cauchy, Weierstrass, and others, that it is desirable to redefine certain concepts and operations. Euler went ahead and summed divergent series, or said this series represents such and such a thing, and wrote down its sum. And we shouldn't do it now; we have different notions of summation. So even mathematics is a growing thing. This is what Dr. Tuckerman was saying. I should like to know whether you consider mathematics a science. Perhaps you have already answered the question and have nothing more to say, and if so, don't stop.

DR. MORRIS: No, I should like to add a point. I protected myself by a qualifying phrase, that the formal sciences can be looked at in two ways, either as sciences of signs (sciences of the formal structure of certain proposed languages) or as tools, that is, as the instruments for making deductions. Now I personally would hold the first as well as the second view. I didn't want to bring that in until now, because it complicates our discussion. There is a sense in which I think mathematics can be regarded as an empirical science, but if so it is a science dealing with the formal interrelation of signs. As an actual fact, whenever the mathematician thinks, as Peirce* stressed (and Peirce was the one who held the view of science as a self-correcting process), he either has marks on paper or on a board, or has some type of imagery or imaginative diagram. Now it is possible to regard a demonstration in mathematics as an empirical process performed upon signs, though it would be different in important respects from the empirical process in science and chemistry. Still you would have mathematics within your linguistic sciences in this sense: in a demonstration, what you have are certain initial combinations of signs we would call postulates. Then you have certain rules of procedure, certain operations that are normally taken over from logic--certain techniques for deduction, certain rules for substitution, and so forth. You have a technique in which you make certain substitutions and operations upon your initial signs. Then you finally come upon another sign combination which you call your theorem, and your "Q.E.D." might be regarded something like a statement in chemistry. If you take some starting point and do certain things, you come out with such and such results. You may find that if you take your sign combinations, which we call axioms in Euclidian geometry, and act upon those with the substitutions of the type you are permitted within the logic you are using at the time, you will come out with such and such a sign combination. If you want to regard mathematics in a sense as within empirical science, then I think you must regard it as a study of the formal structure of certain languages or certain sign combinations, and I think you can do that. If you can do it, mathematics too becomes a self-corrective process. You might find that Euler made a certain kind of mistake in manipulating his signs according

* C. S. Peirce, Collected Papers, vol. II, (Harvard University Press). This gives his views on signs, logic, and mathematics.

to rule.

DR. DEMING: But he got the right results.

DR. MORRIS: That might be accidental. I would make a sharp distinction between the empirical sciences that do not deal with signs (such as chemistry) and certain sciences which do deal with signs. The mathematician, as a mathematician, is, from my point of view, studying and attempting to make true statements about the formal structure of certain proposed languages. He is interested in languages, the type of languages which the other scientists can then use as instruments of deduction. So if you make them both empirical sciences, you have to distinguish the objects about which they are empirical. In one case they are signs and in the other case they are not. In general, mathematics would be brought into science as part of linguistics rather than as part of the general theory of nature.

MR. GLICK: Since logic and mathematics, as a tool, enable us to go from one set of singular statements to other singular statements, and enable us to arrive at a basis for predictions that we didn't have before we had those tools, wouldn't there be some natural connection between the methods of logic and mathematics, and the external world out of which we have derived our singular statements?

DR. MORRIS: I would reverse the picture. Because of the function which logic and mathematics must perform in the process of deduction and prediction, they must be controlled in the process; and it would be the same way about any other tool. You might try to think of what sort of harmony exists between a microscope and nature, but the point is that the microscope is controlled in terms of the functions it must perform, and our logical and mathematical processes are similarly controlled. So I would not start with just nature, which is barely reflected in mathematics and logic, as Peirce tried to do. He remarked that mathematics and logic should correspond with nature. I would rather look at it the other way around. If you want to avoid deducing false statements from true statements (and the predictive element demands that), you will in time eliminate those processes which do not conform to that requirement, and what you have left in your logic and mathematics are processes that give true singular statements from true singular statements.

DR. EZEKIEL: I just want to ask one question about your discussion a moment ago between mathematics and other sciences. Take the branch of statistical mathematics, particularly the theories of probability; don't you get into still another border line: probability,being not completely deductive,but in part inductive can fall part way between formal mathematics and the observational sciences.

DR. MORRIS: I do not think it is different from the situation in geometry. You can conceive of it as a part of empirical science. In other words, part of the theory of probability is just part of mathematics.

Mathematical probability is not any different from any other branch of mathematics. The calculus of probability has the same kind of mathematical character that any other kind of calculus has. It has the same logical status as any other branch of mathematics. On the other hand, when you attempt to make an interpretation of that calculus in one form or another, as in terms of the frequency theory, where you make correlations between your abstract calculus and certain empirical happenings, it is the same thing as taking the abstract system of Euclidian geometry, and correlating lines and points and so on with actual objects in the world. Your calculus is transformed into an empirical theory. It is a question whether the world is Euclidian or not. When you take the calculus of probability and compare it with the frequency with which events happen or propositions are true, then you have given it this type of empirical correlation. The thing has taken on something of the empirical character of science. I doubt whether the theory of probability raises any special logical distinction. Did you think that perhaps it involves a new distinction that we have not been taking account of?

DR. EZEKIEL: The only way it possibly involves a new distinction is that it is a theory based in part on happenings, empirical in that sense. Perhaps one could say that Euclidian geometry applies to almost all happenings on the earth. It is a much more generalized state of empirical happenings than in chemistry or physics.

DR. MORRIS: So far as I can see, the theory of probability does not raise any new logical problem that is not raised by mathematics or empirical sciences. Some of you perhaps are familiar with some of the monographs that are beginning to come out in the _International Encyclopedia of Unified Science_.* I mention them now because one recently has come out by Ernest Nagel, _Principles of the Theory of Probability_, which discussed the process of nature in terms of what you are raising; that is, the various ways in which probability could be determined empirically in the calculus, and so forth. Also there is a monograph by Rudolf Carnap on the _Foundations of Logic and Mathematics_; it is one of the most illuminating accounts on the nature of mathematics and logic and their relation to the empirical sciences. You will find the account I have given here is in the main of the type given in that monograph.

DR. SEEGER: You have spoken of the science of science. Would you agree that there is also an esthetic side of science?

DR. MORRIS: An esthetic side of science? I would say yes. I don't see any object whatsoever that is not capable of being an object of esthetic precision. It seems to me that whenever we are attentive to value characters of objects as such, we are looking at those objects esthetically. As you know, the mathematician takes peculiar delight, unholy delight from the point of view of some of us, in seeing how condensed

* Bibliography on page 172.

and elegant his presentation can be. His dream is to have something that will revolutionize the world on a page. The shorter it gets and the more elegant, the greater he is pleased.

DR. DEMING: Not only that but he likes to see his equations set up in beautiful style, symmetrical and so forth. Often he accomplishes his artistic aims so well that a copyist or printer, untrained in mathematics, will discover errors in manuscripts.

DR. MORRIS: Yes. There is no reason whatsoever why one cannot take an esthetic attitude toward anything. It can become an object of poetic discourse. You can write a poem about it. Edna St. Vincent Millay has written a sonnet, "Euclid alone has looked on beauty bare," in which she took beauty itself as an object of esthetic discourse.

I had a very interesting afternoon with Martha Graham, the dancer, in the collection of mathematical models at the University of Chicago. I doubt if anyone had ever looked at those mathematical models as Martha Graham looked at them. She was perfectly fascinated by them from the point of view of the esthetic properties of formal structure, whereas I doubt if the designers had that attitude in mind,--theirs was a scientific attitude preeminently.

I also think there is nothing that happens that cannot be an object of scientific discourse. For instance, the scientist could investigate why Martha Graham was interested in these objects as she was, or why Edna St. Vincent Millay wrote this poem when and as she did. There is nothing that happens that cannot be an object of esthetic discourse, or scientific discourse, or technological discourse.

DR. KELLOGG: I have seen a distinction that may be considered crude by a person that deals with logic, but which seems to me very useful: science deals with the relation of facts to facts, whereas the esthetical deals with the relation of facts to man; and morals,--the relation of man to man. Those three great classes of relationships are drawn.

DR. MORRIS: Yes, I would think that is roughly correct. As it stands it is not a very precise scientific formulation. But from the point of view of technological discourse, as a useful way of getting a certain rapport between us, it is perhaps an effective statement. It suggests what I meant when I said yesterday that art presents things "interest-wise," in terms of the value they have to some interested party.

DR. KELLOGG: That is the big trouble in all science now; some of these so-called new and border-line sciences have ceased to be objective. Their participants react emotionally.*

* See also Dr. Kellogg's remarks on p. 180.

DR. CHAPLINE: Is the chemistry of soils one of those emotional sciences?

DR. KELLOGG: Well, there are certain phases of soils that fall within that category, I think.

DR. CHAPLINE: It seems to me that from a practical standpoint of the folks in the Department of Agriculture there are certain phases of science that approach very closely to what Dr. Kellogg has just said. Apparently we are not always dealing with science, but we are sometimes dealing with technology. We have often assumed that we were scientists and are dealing primarily with science in attempting to present our results in scientific discourse, with a certain amount of technological interpretation...

DR. KELLOGG: With our minds on some social and economic objectives.

REMARK: Most of our efforts in the Department of Agriculture, I would say, have a certain amount of practical application in the background, or a practical problem that we can deal with.

DR. KELLOGG: A man can be practical without necessarily being emotional.

REMARK: I am not thinking of being emotional, particularly, but we are at least dealing with technological problems as they have been defined here. Part of our discourse we attempt to make scientific, and then other parts must, from the very nature of our work and the possibilities of making use of it, be technological. I think the bulk of our effort is the latter.

DR. KELLOGG: I think what you mean there is that we reach the objective through a scientific jargon with a nonscientific approach.

DR. MORRIS: I would agree with the first formulation. My guess is that most of the statements made by you men in the Department are statements in technological discourse, but as you point out--and that is what I tried to stress yesterday--the only control that we ultimately have over our technological discourse is through science. Let me take a very crude illustration. Suppose there is a certain rule that surgeons would pass down to their internes, about operating in such and such a way on such and such a disease. Now the assumption back of it is that this is the most effective technique for realizing maximum recovery, the aim of operative medicine. Suppose that a scientist were to find by a scientific investigation, that some hitherto disregarded technique actually realizes the aim of recovery more adequately than the existing techniques; that statement would be a scientific statement: procedure A realizes the end B more effectively than does procedure C.

That would be a scientific statement. If medicine were alert and flexible, its technique would change as soon as possible, so new technological discourse would be issued by the surgeon to the interne as to how to operate; and here you have the scientific control of technological discourse. The only rational control of such discourse is through science. So I think an important point of part of the activities of the type that would be undergone here is to see that the technological discourse is intimately related to the sciences upon which it can be based and controlled; and in one respect you therefore are scientists and in another respect you are technologists.

A friend of mine offered a good example of the control of technological discourse empirically in a primitive tribe. I mention this because we often have a notion that the primitive react differently. We have heard from the French anthropologists about the "pre-logical and mystical mentality" of the primitive man. We have a greater proportion of technological statements that are controlled empirically or scientifically; but primitives do it too, even if not to as great a degree. This was the illustration: my friend was making an analysis of certain rain-making ceremonies of American Indians. He requested the medicine man to repeat the procedure by which they were to get rain. This particular man repeated it but left out a certain section of the technique, a section that consisted of lighting fires at night in different regions, and rushing out at the first streak of dawn to put the fire out. My friend said, "Why do you leave it out?" The reply was, "The truth is I don't take any stock in it. One night we built a fire on the top of the hill, and we forgot the fire and didn't put it out, and the rain came down just the same." He said, "The next time I had difficulty getting my men to do it, but I had them leave two or three fires and the rain came down just the same." So he left out part of the ritual. His technological discourse had changed through observational control. When we call him superstitious we only mean that we think further investigation would control other parts of the technological discourse that he hadn't yet controlled. I haven't any doubt whatsoever that five thousand years from now an analysis of many of our technological procedures will be found to have in them just the same element of emotion or magic, that we are now laughing at in this medicine man.

DR. DEMING: Dr. Morris, the time is getting short and I dare say there are many questions yet to come. I think it would be well in rounding out this discussion if you would say a word on the confirmation of a scientific prediction. You said that the second characteristic of a scientific statement is that the prediction that it makes can be confirmed. Who is going to say whether the prediction is really confirmed? You may write down a law, "force equals mass times acceleration," or some such thing, or predict a linear relation between two variables, but you never see it confirmed absolutely. There are always errors of observation whenever you observe. There must always be some give and take between theory

and experiment.* Who is to take the responsibility of saying that the law is confirmed, when someone else could argue that it is not?

DR. MORRIS: Well, the scientists are to say. Scientific knowledge is inter-subjective. I think that Peirce was the main figure to stress this view, and by the way, Peirce did much of his work in connection with the government, not in an academic career. Peirce seems to me to be the finest philosophic mind that America has produced. I think he is one of the half dozen great logicians of all time. He stressed the essential character of the scientific mind as probabilistic. I think that fits in with the widespread view that confirmation is always a matter of degree; it is never a question of regarding any statement as completely confirmed.

DR. DEMING: Does it come down finally to human judgment, that somebody must be the judge, and take the responsibility?

DR. MORRIS: It comes down to something more primitive than judgment: whether an expectation is thwarted or satisfied. Suppose you order beefsteak in a restaurant. As soon as the beefsteak is ordered you begin to act in a certain way. You are now ready to handle a certain kind of object, to get such and such a taste. You are all set for a certain kind of object. If a creampuff is brought in, all the expectations you had and modes of procedure you were ready to adopt are thwarted, blocked, or inhibited. Whereas, if it is a beefsteak that is brought in, these attitudes are simply aroused to overt activity. This may seem very crude, but I think that such is the matrix of all confirmation: signs set up certain expectations which are then either blocked or realized by the appearance of an object with such and such a character; the degree to which the expectation and prediction which we have formed on the basis of these signs are realized by the presence of the object determines whether we have a confirmation or not.

There is no absolute confirmation. It can be shown in fact that from any singular sentence it is possible to deduce an infinite number of singular statements. Even the statement that this is a table is not subject to complete confirmation, because from the statement that this is a table, by the use of logic and the laws of science, you can deduce an enormous number of statements and analyze a number of statements which would be true if this is a table. If it is a table, then we would mean, for instance, that we can get certain kinds of tactile sensations from it. We can do certain things to it. We mean that other persons will verbally react and report certain things about it. So we really have something like this:** if p, then q,r,s,t,u,v, ... The question is

* Cf. Prof. Cohen's remarks on p. concerning curve fitting.

** Cf. L. S. Stebbing, A Modern Introduction to Logic (Thomas Y. Crowell, 1930) pp. 304-5

where are we going to stop in our confirmation that it is p? The answer actually is that it depends upon the purpose we have in hand. For some purposes, as if I simply want to write a letter, then the degree of confirmation required is relatively slight. I pull up a chair, lean on the desk, and begin to write on it. It is a desk. Suppose at that point I begin to doubt my own psychological process and become aware of hallucinations. Then my confirmation would not be sufficient. I would ask you whether it is a table. Then of course you might also have hallucinations, so I might have to control that. Some place along the line the confirmation will be adequate. As my friend T. V. Smith likes to put it, the ultimate logic is the logic of fatigue. At a certain place with reference to what you have in mind any more confirmation is fatiguing, and the statement is taken as confirmed; but there is no absolute confirmation.

DR. DEMING: I am sorry to interrupt, but I am sure when we carry this written record to the Under Secretary, who unfortunately is still not able to be out, he is going to be disappointed if we don't bring up the thing that you talked about during the last part of your lecture, where you said that scientists and science are in the hands of non-scientific men. I should like to get your point of view on the question, should the situation be otherwise? If so, how can we as scientists do something about it? Maybe I misunderstood you.

DR. MORRIS: I hesitate to begin to speak here on this because I have some rather grandiose thoughts on that point. I do not know how far they are scientific and how far esthetic or technological. My own emotions begin to operate at this place. You may remember a sentence in Nietzsche in which he asks where the "new nobility" is to be found. I think that as we look around at our fellow scientists, we do not take them to be the paragon of mankind; and I must admit that the scientists of my acquaintance are not always my picture of the new nobility; but on the other hand I do think there is something in the scientific process and scientific habit of mind that potentially is capable of furnishing the core of the mentality of the future. As I indicated, I think there is a vast sociological struggle going on, and that that struggle will decide whether science is or is not to perform that role.

It is perfectly clear that technological discourse is never reducible to scientific discourse. I mean there is an element of decision involved in technological discourse. Life is always more than knowledge. The question is, how far are we going to demand that all technological discourse be corrected and based, so far as possible, upon the most accurate kind of scientific type of knowledge? I take it that when you say science frequently is in the hands of non-scientific minds, you don't mean to imply that human destiny should be wholly in the hands of scientists, but rather that we want the people who are not scientists, that is, who are technologists, to be constantly revitalized and cleansed in the light of the available kmowledge as to whether the techniques they employ and advocate are efficacious or not. In other words, we do not

want the technologist to determine in advance that certain technologies must be carried out regardless of the bearing that science has upon them.

And now the question as to whether the future of mankind will be one in which the scientist is able to invade every field and to furnish a universal corrective to decisions and to techniques, is a crudial one for science. If it is not to invade these other fields, then science will be blocked in its own development; and as scientists we must all be interested in the development of science. You see concretely how a scientist can be limited in certain spheres. The men in control don't always want their commands aired in terms of the scientific material upon which they are supposed to rest. That inevitably means that science becomes subordinated to the position of an instrument to be used when it is convenient and discarded when it is not. It seems to me that such a state of affairs is not compatible with the furtherest growth of the potentialities of mankind. And so I think that the question of how far the scientific habit of mind can function in society, so that society will make use throughout its whole structure of the scientific method and results, is enormously important; and the next decades may very well determine whether science contains within itself the essential seeds of the new mentality, or whether science will simply become one more instrument for the use of whatever purposes and powers happen to dominate at the moment.

DR. TUCKERMAN: I want to present one question. You implied last night that science had in itself some means of determining what were the objectives. Now science says "If you do certain things certain things will happen, if you don't do certain things, certain other things will happen." Technology says, "I want to do this sort of thing," and the scientist, "How shall I do it?" The question is, wherein can science say these are things that are worth while doing?

DR. MORRIS: Science cannot say that.*

DR. DEMING: I hope you will forgive me if I disagree. When a decision is required between two choices of procedure in life, it should be possible, if the choice is of any importance, to arrange an experiment that will decide which one of the two choices is the better, in the sense of which one leads to a more abundant life, or to greater satisfaction as Professor Conklin would put it. History provides a record of countless unplanned experiments of this nature. Surely the central objective in the medical sciences, and I had supposed so also in other sciences, is to find out which way or which procedure is best, in the sense mentioned. Whenever an experiment cannot be planned to answer the question, "Which things are worth while doing?" then the choice is only a matter of taste.** Of

* Dr. Conklin gave substantially the same answer on p. 74.

** Cf. the remarks at the top of p. 75.

course, science does not tell us that mankind "ought" to seek the greatest satisfaction, and doubtless that is what you meant when you said science cannot say these things.

DR. TUCKERMAN: How shall this be determined?

DR. MORRIS: That wasn't exactly my point. My point was not that science could itself scientifically state what values "ought" to be stated, because the statement that "X ought to be done" is a statement in technological discourse and not in scientific discourse.

DR. DEMING: How are those going to be determined? Are you going to say?

DR. MORRIS: I admit there is an element of decision, an element of choice, an element of fiat, that is inescapable. It is inherent in the process of living. Living is more than knowledge.* Decisions must be made; courses of action embarked upon. From the statements of science you cannot mechanically deduce what decisions men ought to make. But decisions, even if they are not determined by scientific material, can be informed and enlightened by scientific material. You cannot deduce any statement in technological discourse from a body of scientific statements. That is a logical thesis which itself would need discussion; I have stated it technologically for the moment. That is, from a scientific statement that can be confirmed, you cannot logically deduce a statement within technological discourse. Therefore, from science alone you will never deduce what should be done.

I want to deny the belief that men have some fixed set of values which they use or have in all cases, and that these are impervious to a knowledge of the conditions under which these values can be reached and maintained. In other words, I maintain that man's decisions are a function of a great number of things, including his knowledge of the difficulties and possibilities and ways of realizing certain ends or values, and that therefore in the bringing of scientific knowledge into the process of making decisions, men's values take on a form or direction that they would not take on without that knowledge; it is not that values are uniquely determined by knowledge. But I want to oppose the educational movement which attempts to represent that there are two great classes of knowledge, scientific knowledge and wisdom--wisdom to be found in the traditional philosophers. You should read Plato or somebody else and get wise; having become wise, you .. know what to do, and science will then tell you how to do it. We are told that there is no process in wisdom. Wisdom is final and complete once and for all, but science moves on in its humble ways, and gives new means for attaining ends that it does not affect. But on the view I have expressed, there is no body of wisdom that is impervious to scientific knowledge and

* Dr. Conklin gave substantially the same answer on p. 74.

can of itself be left to guide human destiny. No one knows what man will want to do a hundred years from now or five thousand years from now, and to say there is a fixed body of immutable wisdom seems to be to be false to the actual picture. We must of course, make whatever use we can of the way men have decided to live, but this does not free us from the need of making our own decisions in the light of our problems and the available knowledge and techniques.

I have a very firm opposition to this growing educational attempt to subordinate science in the cirricula of universities on the ground that it really cannot do anything but implement ends which we must get from elsewhere. As soon as you admit that, the self-appointed "men of destiny" will tell us where the other ends are to come from and what they are. From my point of view, there is no body of wisdom that is or should be impervious to control by science. There is no technological discourse which is not subject to control by science, as soon as we raise the question whether or not the technique is adequate for the attainment of the end in question.

I have great belief in the significance of the scientific mentality for the future of mankind. On the other hand, I firmly believe we must avoid an emotional attitude of venerating science. The scientist is not the new nobility, but he might be an element in it if he learns that art and moral activities do not have to apologize to him at all. Esthetic discourse is just as important as scientific discourse, and men need motivation for action, as well as knowledge. If the scientist can be humble in not ridiculing every form of culture that is not his own, humble in realizing that his is only one activity among human activities, and if he yet can see the important function that science performs with reference to all other activities, in his humility the scientist can become part of the new nobility.

Ten Lectures and Discussions on

SCIENCE: Its History, Philosophy,

and Relation to Democracy

held at the Department of
Agriculture Graduate School
under the chairmanship of

M. L. Wilson
Under Secretary of Agriculture

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Edited by W. Edwards Deming
Assistant Director of the Graduate School

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Lecture VIII

SOCIETY AND ORGANISM

by

Walter B. Cannon
Professor of Physiology, Harvard Medical School

The Graduate School
The Department of Agriculture
Washington
1939

Lecture VIII

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In the auditorium

I

We are composed of extraordinarily unstable stuff, perhaps the most unstable stuff outside of high explosives. The eye is affected by an amount of energy that is about three-thousands of the amount required to affect the most sensitive photographic plate. If the blood supply to the brain is shut off for only an instant we lose consciousness. The amount of energy that passes along a nerve fiber is extraordinarily slight compared with the explosion that takes place in the muscular contraction which results from the passage of the impulse. These are illustrations of the instability, the high degree of sensitiveness of parts of the body. When we consider these high degrees of irritability of parts of the body and realize that the body presents an open system, a system in which energy-yielding material is taken in from the outside and a system which delivers energy to the world about us, and, furthermore, that it is a system which develops from small size to large size--when we consider these things all together, the fact that it continues in action and effectiveness for many decades seems almost miraculous. It not only continues in existence in this way, but it stands knocks and cuts and bacterial invasions and all sorts of internal stresses imposed upon it from the outside and from our own activities.

Now, if we survey the invertabrate and the vertebrate ranges of animals, we find that this ability to maintain stable states has been gradually achieved. How is it that there has been developed this remarkable capacity of our organism to keep stable states in spite of the fact that we are subjected to all sorts of exigencies and variations in the outer world, and all sorts of activities within our bodies which might disturb that stability? It is due to the fact that there has been evolved what has been called by the great French physiologist, Claude Bernard, an "internal invironment," or what I have called a "fluid matrix." Those are terms that require definitions. We ordinarily think of ourselves, you and I, as air-inhabiting animals, but if we consider for a moment the actual condition of affairs we note that we are shut off from the air that surrounds us by dead stuff. Over most of the body there is a horny scurf of skin, dead epithelium; and where that is not present, as on the moist surfaces, there is a layer of dead watery mucus. All that is alive inside these lifeless coverings is bathed in fluid. That fluid is the flowing blood in the blood vessels and the so-called lymph, the watery liquid lying outside the blood vessels and serving in its boggy character as a

means of transfer of material between the flowing blood and the living cells. We can regard ourselves, therefore, as water-inhabiting animals in the same sense that animals living in the ocean can be so regarded. All that is living is immersed in fluid. Now stability, constancy, is preserved in the living parts because there is constancy and stability in the fluid matrix.

I can illustrate some of the favorable conditions that have resulted from the development of these stable states. For example, there are devices in the organism that maintain uniformity of temperature in the flowing blood and in the more or less stagnant lymph. If the temperature tends to rise, there is an automatic device which causes the blood vessels of the surface to dilate so that there is a larger amount of warmer blood delivered to the surface for the diffusion of heat into the outer air. If that doesn't prevail as a means of keeping the temperature from rising, there is a pouring out of sweat. Sweat cools the body much as the evaporation of water through the walls of a porous jug cools the water inside the jug, and the blood then passes close to the skin that has been cooled by this process of evaporation. If the temperature tends to fall, there is a contraction of the blood vessels, a contraction which prevents the warm blood in the interior from getting near the surface, and a shutting down of any secretion of sweat which might be causing loss of heat from the body in that manner. If that doesn't prevail, there are devices that are set at work to increase the production. There is a little gland just above the kidney which secrets a substance called adrenaline, and that substance, circulating in the blood, is capable of very greatly increasing the rate of the burning processes in the cells, so that a greater amount of heat is produced. And if even that doesn't prevail, there is a further resource, muscular contraction, shivering, which is an automatic way of warming the body by means of the heat resulting from the contractile process in the muscles. We have, therefore, in these devices arrangements whereby the temperature is kept fairly uniform, whether the external air is warm or cold. I told you a moment ago that these devices have been gradually developed.

During the months which have passed the frogs in the pools have been down in the mud at the bottom waiting for the ice to melt so that they could rise to the surface and go on with the activities that they manifest during the warmer weather. They have no means of maintaining constancy of temperature and therefore take on the temperature of their surroundings. When, in the autumn, the water reaches a certain degree of coolness, they sink down to the bottom of the pool and there they remain until the spring comes. Reptiles, somewhat higher in the vertebrate series than frogs, still are limited by having no means of maintaining uniform temperature when there are variations in the outer atmosphere. They must hibernate during the winter. They have, however, evolved another device, that of preventing a loss of water from their bodies; consequently they are freer, they do not have to keep near the pool, as the frog must in order to maintain his internal fluids.

They can live even in the dry desert.

I give you these illustrations merely to indicate that there has been a gradual evolution of greater and greater control over the fluid matrix which the higher organism produces, the internal environment in which it lives, and that just so far as there has been developed a control over this internal environment, the organism is freed more and more from the limitations that are imposed upon it by the outer world.

Another illustration: We ordinarily have about 100 milligrams of glucose or grape sugar in 100 cubic centimeters of blood. We say 100 milligrams percent. (I am using now the figures given by the older methods of determining blood sugar.) If that concentration drops down to about 70 milligrams percent, there appears a peculiar reaction accompanied by sweating, by a rapid heart beat, by a dilation of the pupils and other indications of activity of the so-called sympathetic nervous system. The activity of that system is associated with a protective liberation of sugar from the liver. If that isn't effective and the blood sugar concentration goes further down, to 45 or 50 milligrams percent, about half the concentration that ordinarily prevails, there is likely to result a convulsive state, a state which is associated with a much greater discharge of the nerve impulses, and which results in a greater liberation of sugar from the liver. On the other hand, if the sugar concentration rises from 100 milligrams percent up to 170 or 200, then the sugar escapes from the blood through the kidneys and is lost.

When the body temperature varies, there is the danger that it may rise to 108 or 109 degrees Fahrenheit, and bring about in certain brain cells changes which are irremediable; and if the temperature should fall too far, there is a danger that the processes in the body would all become much slower than they are ordinarily and we should become very much less efficient. When the sugar concentration varies to extremes in the blood, we are exposed to the danger of convulsions, on the one hand, and to the loss of that material, and all the work that was necessary to bring it into the body, on the other hand. Neither of those conditions is allowed. Corrective activities are set at work in the organism to prevent the onset of convulsions when the blood sugar tends to fall too low; and ordinarily there is a means of preventing an overflow through the kidneys.

Take other illustrations. The concentration of calcium in the blood is about 10 milligrams percent. If it goes down to five milligrams percent, convulsions are very likely to come on; and if the concentration is doubled, i. e. if it goes up to 20 milligrams percent, the blood becomes so thick, so viscous, that it hardly will circulate. These conditions do not occur. There are devices in the organism that prevent these things from arising. Again, as many of you know, there is a very nice balance in the body, in the fluid matrix, between an acid and an alkaline reaction. Ordinarily the blood is slightly on the alkaline side.

The pH--some of you will recognize that term*--is about 7.4. Now, if the reaction of the blood becomes even to the slightest degree acid, if it drops down to 6.95, coma comes on, consciousness is lost. If the pH figure goes up from 7.4 to 7.8, convulsions are very likely to occur. We go through our daily lives, therefore, with the danger, on the one hand, of falling into coma, and with the danger, on the other hand, of going into convulsions. Yet these are dangers that we are utterly unaware of, because there are adjustments in the organism which prevent these harmful conditions from appearing.

Let us note what occurs when a vigorous young man undertakes energetic exercise. We will say that he is doing a mile run. That demands sugar, because muscular work is done preferably with the use of sugar as a source of energy. If there is continued muscular labor for a long time, there may be utilization of the sugar stored in the liver to such a degree that it is largely depleted. There is the danger, of course, that sugar might be reduced in these circumstances so that the convulsive state that I spoke of earlier might appear. But that is not permitted. Also, in the course of extreme muscular work there is a large amount of acid produced, non-volatile acid, lactic acid. Enough lactic acid could be produced to overwhelm the alkali in the blood in a very short time, and thereby to change the reaction of the blood in the acid direction so far that coma would come on. Well, you don't see the young men who are engaged in vigorous physical effort--in intercollegiate competitions, for example--you don't see them falling into coma. There are protective reactions, set at work immediately and automatically, to prevent that disastrous situation from developing. The lactic acid has to be burned to a volatile acid which the body can readily get rid of. The reactions which take place are all directed towards supplying enough oxygen so that the lactic acid can be burned to volatile carbonic acid.

The moment one begins to move, the heart beats more rapidly and the blood pressure begins to rise because there is a contraction of the blood vessels in the very capacious area of the abdomen. The blood vessels of the laboring muscles dilate so that the blood goes through them in larger volume. The general rise of blood pressure results in a faster flow of blood from the lungs, where the red blood corpuscles, the oxygen carriers, load up with oxygen and carry it to the tissues where the carriers unload and take on carbon dioxide which they transport to the lungs for discharge from the body. Accompanying these changes there is a contraction of the spleen, which is a storage place for red blood corpuscles concentrated there, so that not only is there an increase in the rate of movement of the corpuscles from the lungs to the active tissues, and from the active tissues back to the lungs again, but there is a larger number of these corpuscles set at work.

Again, vigorous exercise is sure to be accompanied by a great increase in heat production. The body as a working mechanism has an efficiency of only about 25 percent, that is, of the energy turn-

* pH = the negative of the logarithm of the hydrogen ion concentration expressed in moles per liter.

over in muscular activity, about 75 percent appears as heat. As a consequence of this heat production during vigorous effort there is a danger of the temperature rising to such an extent as to be harmful to the organism. But that doesn't occur. A person who begins to work vigorously also begins to sweat, and as he sweats his skin cools off in the manner I have already described; the blood from the warmed muscles, warmed because of the extra heat produced by their activity, when carried to the cool skin, loses its heat to the skin. Thus the temperature is kept from rising to a serious height. Some years ago I calculated that if the oarsmen in a four mile race, who are using all their muscles to the greatest possible degree, could not get rid of the heat which they produce while rowing, they would be cooked by the end of the third mile! That state isn't even approached because there are the self-corrective arrangements in the organism which promptly protect against that disaster.

II

Well, these are some of the dangers that we are constantly exposed to and which we aren't ordinarily aware of and which we avoid because the fluid matrix, the internal environment in which our living parts reside, is maintained in a fairly uniform state by automatic devices. Now those devices can be classified according to whether they are concerned with supplies or with processes. We are ordinarily accustomed to taking in our supplies by three meals a day. The energy yielding material which these supplies give us is being used up all the time, continuously. How is it that the body manages to provide for this continuous use? It manages by processes of storage and release--storage in times of plenty, and release in times of need. When we eat a meal having an abundance of starchy food, that starchy food is changed to blood sugar, glucose. In the process of digestion, glucose is taken into the blood. It may mount up to near the danger line of being lost through the kidneys, that is, about 160 milligrams percent. It is then first stored for a time in the loose connective tissue of the body under the skin, the regions that give us a water-blister when we have an injury to the skin, and later it is gradually stored also in the form of animal starch in the liver. There it stays until it is needed. If there is need, on account of vigorous physical exercise, for example, it is brought out from storage. Also, if we have to engage in great muscular effort, as, for example, when we are afraid and may have to run, or we are angry and may have to fight, the sugar is likewise released from storage. Storage, therefore, is the means of adjustment between plenty and need.

When constancy is a question of processes, the stable state is preserved by altering the rate of the process. We are constantly losing heat. Every one of us, at the present moment, is losing heat to the outer world because we are constantly producing heat by contractions of the heart, contractions of the diaphram, activities of the liver, and so on, and we must get rid of it. The rate at which heat is passed through the skin, however, can be varied. If a cold draft should come into this room

now, the process of heat loss would be promptly shut down by a contraction of our surface vessels; and if we had to get rid of extra heat because the surroundings became warm there would be a dilation of the surface vessels, a larger amount of heat would be brought to the surface by the warm blood, and thus the rate of heat loss would be increased.

III

The dangers that I have described, dangers which might result from the failure of regulatory devices to operate, we know about only because the studies of physiologists have shown what they really are, and what the natural devices are which prevent their occurrence. The important question arises as to what value all this has for our conduct. The value arises from the fact that there is freedom because of the absence of any necessity of paying attention to conditions in the organism that might make trouble. If we had to go through life saying, "I can't pay attention to what you are saying now because I have to see what my blood-sugar is. If it is too low, I must get out some more sugar and forestall a convulsion that I might have if I don't attend to it promptly." Or, "I have to be careful about too much acid in my blood and make sure that I am not going into a coma." All these protective and corrective arrangements have been gradually evolved in our bodies through hundreds of thousands of years of hard racial experience. They have more and more liberated us from the vicissitudes of our external environment and from the disturbances that we produce in our internal environment by our own activities. They have made it possible for us to go on doing the work that we want to do, doing vigorous work which produces heat, which uses up sugar, which develops acid, doing that vigorous work without having to pay attention very much to what is going on inside us. Automatically, all this is managed.

The question that I want to raise is whether there aren't some general principles of organization, whether the body through this long, long experience hasn't learned certain lessons that may be useful to us, whether the body biologic hasn't some suggestions to offer to the body politic. The resemblance between the body biologic and the body politic, the similarity between the two, has long been noticed, from Bible times, through the times of the Greek philosophers, down to the present. The comparison between the two has been in a certain amount of disrepute. Sociologists say they don't get very much out of the comparison. Most of the comparisons of the past, however, have been concerned with analogies of structure. For example, the skin, which is a protective armour, is a fortress. Again, there is the digestive tract, which is a commissary department. There is the digestive process, which is a sort of kitchen. And then there are the white blood corpuscles, which are scavengers, and so on. I don't wish to call attention to resemblances of that sort. I wish rather to lay emphasis on resemblances of organization, and perhaps to arouse interest in the possibilities of applying the experience of the organism, which has learned the trick of stability, to social conditions; and I want to do so by comparing processes instead of comparing structures. The cells of the body, of course, have developed a

very high degree of division of labor and in association with this division of labor they have become fairly fixed in their positions. As relatively fixed units they have become dependent upon organization. The cells that are tucked away in the remote nooks and corners of the organism, far from the surface through which oxygen enters the body or carbon dioxide leaves it, or far from the surface where food is taken in or where the non-volatile waste is lost--those cells must depend upon organization for their well being.

Now the workers in modern society have also become specialized in their activities. There has been a larger and larger degree of division of labor as industry has become more and more complex, and to a considerable degree the workers who are thus specialized become fairly fixed in their positions in the population. They become simultaneously less and less capable of supplying their own needs. They cannot make their own clothes, they cannot grow their own food, they cannot get necessities of other kinds which come to them from remote parts of the country or the world. They are like the cells of the body, dependent upon organization. We know that the complexity of conditions in the organism of the higher vertebrates and in the political or social organization has been the result of a gradual evolution. I have pointed out that there has been a growth of control of the internal environment of the vertebrate organism, which has thereby gradually become more and more freed from the disturbances in the surrounding world and also from internal disturbances, all because of the development of constancy in the fluid matrix. Is there not a possibility that our social organization likewise is undergoing a gradual evolution? May not it be in a state where control over an internal environment is important for maintaining stability? If there could be developed a fair degree of stability, would there not be a consequence in the body politic something similar to the consequence in the body biologic, that is, a greater degree of freedom?

Here I must call attention to a difference between the units in the body politic and in the body biologic, because the units in the body politic have intelligence, and possibilities of using that intelligence in interesting ways. I think that if we consider the body politic as a social organism, we can see that at present in its organization there is much that is subject to very serious criticism. Stability is strikingly absent. There are great oscillations of economic conditions. There are flush times and depressions, and in this state of economic chaos there are tragic and cruel consequences: with grain elevators bulging with wheat, people are hungry; with growers of cotton and wool having difficulty in finding a market, people are in rags; and with factories closed, men eager to work are kept idle. Financiers have tried their hand at settling this situation, and one of the greatest of them, Sir Montague Norman, declared that he regards the situation with great humility, confessing that it is too much for him. The managers of great business enterprises have suggested national economic concils and trade associations. Labor leaders have suggested the shortening of hours and other concessions to labor. And therefore I think we may be assured that the social organization has not yet reached

a condition which is comparable to the organization which each of us carries around within him as a mechanism which has become capable of maintaining stability and steadiness.

IV

What are some of the suggestions which the bodily organization has to offer for betterment of the social organization? What corresponds to the fluid matrix of the body? Well, here I have only suggestions to offer, and they must be taken critically. We must watch our steps carefully in drawing analogies, nevertheless these suggestions may lead to further discussion. It seems to me that the fluid matrix of the social organization is the system of distribution--the system which underlies commerce--the rivers, the canals, the roads, the railroads, the trains, the trucks, all the arrangements we have for distribution, including the wholesale and the retail purveyors in this vast and intricate stream of commercial distribution. In this stream, manufactured goods are put in at one place and carried to another place; farm products are put in at their origin and carried elsewhere. And in the operations of this fluid matrix of the social organization you can take something out of the stream only when you put in something of equivalent value. But it is usually not easy to do that immediately or to provide the full equivalent, we will say, at the same time that you take out a certain amount, and consequently we have money as an extension of the method of exchange; and associated with money, credit. Thus money and credit become part of the fluid matrix of this vast system of distribution of goods from individuals in one part to individuals in another part of our great social organization.

What has the body to suggest further? First, that there should be a constancy of the social fluid matrix, that is, that there should be a certainty of distribution of necessaries, a distribution of food, of clothing, of the means of keeping warm, of assistance in case of injury or disease. That is what happens in the body, and there is the hint that it should be true of the social organism as well. This doesn't mean a rigid social system, but one that is adaptable, one that would prevent a glut of supplies at one place and a lack of them at another. It could be managed by storage of supplies in times of plenty, and their release for distribution in time of need, precisely as in the organism. And if processes need to be altered so as to bring about constancy of the social fluid matrix--that is, constancy in the commercial stream--then processes, labor processes, for example, might be augmented in their rate in certain circumstances and decreased in other circumstances.

Another suggestion is that stability is more important than economy. The body throws away water, if there is excessive water taken in. It throws away salt if an excess of salt is taken in. If there is too much sugar, sugar overflows the barrier presented by the kidneys and is lost from the body. If the body temperature tends to fall, there is muscular activity, a real loss when one shivers, because one does no external work. There is only internal

work, a depletion of material in order to prevent a drop of temperature--a wasteful process, but one which preserves a constant state.

Furthermore, stability is assured by what the engineers call a margin of safety. The body biologic is not built on any skimpy and niggardly plan. We have two kidneys and we need only one. We have twice as much lung surface as is required; surgeons can take out a whole lung on one side and the individual will still live. A large amount of the intestines can be removed; about two-thirds of the pancreas can be taken out; half the thyroid gland. These are illustrations of the fact that the body has arrangements in itself for maintaining stability by a wide margin of safety in its construction.

Another suggestion is related to the sensitive signals of change, possibly dangerous change, in the body biologic. A constancy of the fluid matrix is maintained because when there is a change in one direction or the other there are automatic signals which prevent the oscillation from going to a dangerous degree. If we exercise even slightly, there is an extra production of carbonic acid that stimulates the respiratory center; almost instantly we begin to breathe deeper and get rid of the extra carbon dioxide and also the danger of accumulation of acid in the blood, If the blood sugar goes down to 70 milligrams, there is a signal which results in setting at work corrective devices that liberate sugar from the liver. It is hard to believe that there could not be found, through a careful study of processes in the commercial world, signals that would warn against the great oscillations that we have all experienced. You may recall that in the late twenties everybody was saying, "The United States has just struck its pace. We are going onward and upward. This is the real tempo of our country." And then came the terrible crash of 1929. That sort of disaster ought not to be a possibility; an application of intelligence to the economic situation should discover signals indicating that conditions are oscillating too widely. The mechanisms of stability, I believe, can be learned.

Furthermore, there is the suggestion from the body that there should be a special organized control, a control given by society itself with power to regulate supply and demand, power to store goods in times of plenty and to release them in times of need, power to retard and accelerate processes, power to lay aside wage reserves for temporary unemployment, power to train labor in new skills when there is a necessary change of labor activity. In the body there are these arrangements, and they are automatic. Because they are automatic we are free, free to do all sorts of interesting things, free to engage in activities that are of interest to us, free to play, free to work, free to enjoy art, to engage in discoveries and adventures, free to satisfy all of the spontaneous interests which develop in us when there is no pressure on us to pay attention to the necessaries that are at the basis of our well being.

Finally, the question to be considered is whether stability in the body politic may not have the same effect that it has in the body biologic, whether an organization that would provide the stable states that I have described might not favor freedom of the individual. That, I think, is the central question. Can we have a social organization in which we would enjoy the degree of freedom which we now enjoy, and which would show the stability which the body shows? Must an organized society interfere with our freedom to such an extent that each one of us is fixed firmly in place? I am inclined to think that by the application of human intelligence, perhaps by taking the suggestions that the bodily organization has to offer, it would be possible to develop a social organization in which there could be much greater stability than we now enjoy, and along with that, a preservation of individual freedom.

SEMINAR

following the lecture by
Dr. Walter B. Cannon

Office of the Under Secretary

Syllabus

1. As units in the social organism, we have intelligence, and that constitutes a striking difference between the cells in the body and the units in the social organism. P. 208; see also p. 203.

2. In a stable organization of society, the members who make contributions would be assured of the necessaries of life, and there would be the same freedom as the body enjoys. People can do their best work when they are free of worry. Leisure for non-competitive interests results in many benefits to society. P. 209.

3. The use of leisure time is a serious problem; p. 211.

4. Regimentation would not be necessary in the stabilized society, except that organization would be required for teaching workers new skills when new inventions appear. P. 209.

5. The body is not a dictatorship. All our emotional reactions to experience are dependent upon conditions within us. P. 210.

6. The organizations of some of the lower forms of life, such as ants and bees, do not allow free play, and have been rigidly fixed for millions of years. But the brain of man allows his social organization to move more rapidly and to adapt itself to changing conditions. P. 210.

7. There should be freedom of movement inside the organization, but men should be provided with information concerning the difficulties of changing from one job to another. There are persons who have ambitions beyond their capacities, and they should know the obstacles confronting them. P. 211.

8. A closer cooperative system between governments would be a closer analogy of what we have in our bodies. P. 214.

9. Society has gradually become more and more complex and in the course of that evolution it has become more and more free from the disturbances of nature. The preservation of food in storage is one advance. P. 214; also p. 204.

10. Man must devise means of avoiding the extreme shifts of economic conditions that we have seen during the last decade. In the body biologic there are warning signs that indicate danger, and second and third lines of defense will be called into play if necessary, to enforce constant conditions. P. 215; also p. 205.

11. The body often achieves its stability by being wasteful. It is built with a wide margin of safety. P. 217; also pp. 204-205.

Proceedings of the seminar

DR. BLAISDELL: This group, Dr. Cannon, meets regularly Thursday mornings following the lectures in the auditorium for the purpose of following up, if possible, some of the points made in the lecture and exploring the implications of those points, and for the purpose of engaging in an informal discussion generally. We don't have any formal or regular system or procedure in here except that we don't allow anybody to talk too much.

DR. CANNON: (After a pause) Am I supposed to ask the questions, or are others? (Laughter)

DR. BLAISDELL: Well, you see, I said we didn't have any set procedure, so if you feel impelled to ask a question, it would be all right.

DR. WINTERS: Dr. Cannon, you spoke of an analogy between the division of cell work in the body and the organization of society. What could we learn from societies, biological societies, as they work together? We recognize in insects such as bees, certain societies. In our social structure, aren't we handicapped in some cases, maybe helped in others, by our biological nature? Our psychologists have made certain tests, have found certain psychological reactions to be fundamental. Aren't we bound by certain biological inheritances as members of society?

DR. CANNON: I think that the striking difference between the unit cells of our organism, and ourselves as units in the social organism, is that we have intelligence and a possibility of individual adaptation to different circumstances. In the body, of course, most cells are strictly fixed. There are men, I think the psychologists will tell you, who like to be fixed, who like the routine job of every day, who don't like to have anything else to do. They do their daily task, get through with it and come the next day to do the same thing over again. And there are other persons who wish to move about, they don't like to be limited to any special work; they want freedom to get into something else. I suppose there are two ways out of the latter situation. One is to make arrangements for a certain amount of flexibility in the social organization, and the other is to insist that persons who wish to make changes shall make them in accord with a plan, including notification and adjustment, to allow shifts to be made. There is obviously a definite difference between the fixed units of the biological organism and the motile units of the social organism, so far as there is a social organism.

DR. WINTERS: You have a natural and fixed coordination with your bodily organization. In free society we don't have it, except as it strikes the majority of society. You have coordination in your bodily organism and coordination between the specialized cells. Is that due in part to our biological inheritance?

DR. CANNON: I think we have a considerable amount of coordination in our social set-up. You are concerned with the farmers who are doing one thing, and there are other departments in the government concerned with industry and others concerned with commerce. These are divisions in the social organization that are comparable in some respects with the divisions in the body functions.

The question has been raised whether there aren't two types of freedom that were being discussed yesterday. That is an interesting question. The freedom from paying attention to all the details of constancy in our own bodies is a freedom which allows us to make almost every manner of adjustment that we wish with the outer world. We can work as we wish, or play as we wish, in our free time. We can contrive inventions. We can paint or model in clay. There are all sorts of interesting things that we are free to do because we aren't bothered with details of bodily functions. Now, the point that I tried to make at the end of the talk yesterday was that if there were a stable organization of society, so that individual members who make contributions, were assured of the necessaries of life, that condition would contribute to the same sort of freedom that we enjoy as a consequence of automatic management inside our bodies. We should be freed from the worries and anxieties of providing for the necessaries. I think, perhaps quite naturally, of the situation that prevails in academic circles where we have fairly fixed positions. In those positions we have assurance of continued service, and as a consequence there is liberation from anxieties and worries that might arise if security of tenure didn't prevail. Of course, there are persons who would hesitate to think of all society in such terms. They say that if you don't have uncertainty of a job, you won't get work done. I don't think that is necessarily true. In the Army and the Navy, isn't there an illustrative example to the contrary? I imagine that to a considerable extent the situation is similar in the civil service of the government. Is there slacking on the job because of security? I doubt it.

DR. SEEGER: Dr. Cannon, is some sort of regimentation implied as the ideal?

DR. CANNON: I don't think so. Perhaps regimentation to some degree would be required among workmen of routine type--manual laborers. Suppose there is a new invention which suddenly puts a group of them out of work altogether. Well, there are two alternatives. One is for them to suffer, the other is to be organized and be taught new skills, so that they learn something which will adapt them to a new situation. That would require a certain amount of regimentation. On the other hand, in the long run, it seems to me that that would be better than simply being thrown out of work altogether.

DR. SCHWARZ: Dr. Cannon, in comparing the biological organism to the social organism, I think it is quite easy to justify totalitarian states on biological grounds, because in the biological organism the cen-

tral nervous system assumes a dominant position and the other organs come, so to speak, under the direction of the central nervous system. Is it possible to find a similar justification for democracy?

DR. CANNON: I don't think the organism, your body and mine, is an autocratic, totalitarian affair. Rather, it is a democracy. We might think of the brain as in control of the body, but the brain is utterly helpless if there is a momentary check in its blood supply, so helpless that we instantly fall to the floor in a faint. The brain is dependent, immediately dependent, upon a proper blood flow through its vessels. And back of that is a proper supply of oxygen by the working of the respiratory system. And still further back is the provision of food by operations of the digestive tract. Our bodies represent a cooperative organization and not an autocracy.

DR. BELL: Isn't there a definite stimulation of the mind also that comes from all over, from the toes, everywhere, more or less dominating the thing that the brain is going to do? It seems to me there is one of the important features to bring out in connection with democracy, the body is not a dictatorship at all, because the brain is under domination of other parts of the body.

DR. CANNON: Quite so. All of the emotional reactions to experience are dependent upon conditions within us. The one thing in our organization that is in the nature of crime and autocracy is cancer. When cancer cells get busy they dominate the organization and destroy it.

DR. DEMING: Dr. Cannon, do you find in the study of other creatures of the earth, such as ants and bees, wild animals such as the wolves, and so forth, that they are organized more on a biological basis? Do they have the same difficulties as men in the conflict between the individual and the group? Perhaps you have already expressed yourself on this point.

DR. CANNON: I think the ants and bees, for example, are very unfortunate organisms, if I may say so. In some instances the females are all destroyed or the males all destroyed; there are neuters around, and the queen, but the organization is so firmly fixed that I suppose for millions of years there has been no progress. I am subject to correction here by anybody who knows the habits of these forms better than I do. They illustrate, I think, one of the dangers of a social organization which doesn't allow free play of the peculiar equipment of mankind, which is his brain. With this marvelous organ for adaptation and adjustment and discovery and invention there are all sorts of possibilities that the lower forms do not possess. With freedom from worry about what is going on inside us, and freedom from worry about external conditions, I mean the essential conditions for existence supplied from the outside, we can engage in many kinds of activities which are non-competitive and which may be of very great social interest. Charles Lamb was a clerk in the Indian office, wasn't he, when he wrote his delightful essays? If I am not mistaken, Spinoza had an

incidental job when he was at work on his philosophy. And I haven't looked this up, but I have a memory that Hawthorne was in government service when he was writing some of his most interesting work. Then there is the possibility of inventions which would be serviceable to society and which an ingenious person with freedom could undertake.

DR. SEEGER: It seems to me, though, that most of these are concerned with the freedom for rather intelligent individuals, and that our society deals with so many individuals who are not content with just the necessaries, that they want what they call the luxuries, and that would mean a complete redistribution, wouldn't it, of economic values? How is it possible for these people to be free to get their luxuries?

DR. CANNON: I think there you have raised a practical question which is right before us. We are confronted with a situation in which machines have taken the place of manual labor to a very great degree. Consequently, more and more laborers--I am talking now of the group of manual laborers mainly--have less and less to do. That presents a problem of what to do with free time. What shall we do with leisure? The use of free time is a very serious problem, and I don't think it has been properly or sufficiently considered. It is a pressing problem now, and, of course, it would be sure to become more urgent with a larger extent of freedom.

DR. SEEGER: I had in mind also a man that has a position in which he has a certain amount of freedom, but he decides he would rather have another position because in that other position he can have more money to do the things he wants to do. Now it seems to me that is where our conflict comes in. The difference betweem a biological organism and the body politic is this: in biologics a cell in the foot doesn't say, "I would like to be a brain cell;" but there are people who see other people and say "I would like to have that job; I would like to have his house and his motor car." That is where the trouble starts.

DR. CANNON: That question was raised earlier. It is a question of movement inside the organization. It would be important I think, to provide diffusion of knowledge concerning possibilities. Suppose a man does want to get a more remunerative job and there aren't any such jobs, or the difficulties of transferring are so great that he runs a considerable risk if he makes the attempt. Information to that effect, it seems to me, would lead the man to hesitate, and possibly refuse altogether to try to change. There are persons who have ambitions that are beyond their capacities and they don't make the grade anyhow. They should know the obstacles confronting them.

DR. BLAISDELL: Dr. Cannon, your talk yesterday opened up a cosmic horizon for me, if I can use that phrase, because I thought I found in it a substantiation or reason why we could continue to have faith in democracy as a form of government and as a way of life.

Just let me give you my thought process very briefly. If I under-

stood what you said yesterday it was that when we consider the body as an organism and when we remember that it has been evolving through a long, long period of time it had changed from a lower stage into an intermediate stage and then into the higher stage which we now recognize, and that possibly the outstanding characteristic of this higher stage is the state of stability that you referred to and described, and that that made it possible for each one of us to be free of all these physical worries or concerns that otherwise we might have to pay an awful lot of attention to if that state of stability didn't exist. The thing that impressed me was that that had been achieved over a long, long period of time and is a result of an evolutionary process which apparently doesn't stop but has been continuous for a long, long time, and probably will continue. What we call society today is made up of individuals, each being an organism; and the problems that society faces today grew out of the problems of individuals and groups of individuals. I thought I saw in your talk a suggestion that we could continue to hope for an evolution of society to take place in much the same way as the evolution of the individual; and that sooner or later this evolution of society would result in a stable state roughly comparable with the stable state of the individual.

Now if that is correct, it provides for me at least some ground for continuing to have faith in democracy as a form of government and as a way of life. In enabling us to understand the social organism, science thus far hasn't been able to give us as solid a basis of justification for our beliefs as science has given us in our understanding of the physical world around us, and even of the organism itself. In the last analysis, as a matter of faith and belief, we can't prove that democracy is the best way of life; but so long as we have faith in its ability and believe in the possibilities of that form and of that way of life we shall probably continue to give it our best energy and our best abilities. Someone said recently that the most destructive form of "ism" at large in the world today is defeatism; and particularly in the face of the will to power and the will to action that the dictators display, a great many people in the democracies have gotten into the frame of mind of saying, "Oh, what's the use?" But now if science, as a result of its understanding of the evolution of the individual organism can give us a solid ground for saying that society ultimately is destined for a much finer system of organization and stabilization, I think we can continue to have faith in democracy as a way of life, and as a form of government. I should like to have your comment on that. I am afraid I have summarized it very badly and very incoherently.

DR. CANNON: I should like to have you define the type of democracy you are talking about. Is it democracy as a means of getting political opinion, or is it democracy in its economic or social meaning? There are various definitions of democracy which could be considered. What form of democracy do you have in mind?

DR. BLAISDELL: Well, of course, that throws the question back at me. I am willing to catch it and throw it back again. In using the word "democracy," I mean a representative form of government, a democratic repre-

sentative form of government, in which the individual has rights that are recognized and for which institutions have been established and are maintained. To me, at least, this means a form of government, a way of organizing our political life for getting things done, which takes into account the opinions of the majority of the able-bodied adult people in the community, and at the same time allows for free individual development.

DR. CANNON: Well, in those terms, it seems to me that one could regard the present world situation in governmental aspects as one in which there are two or three cancers that are troubling us, governments that are so disregardful of the welfare of society as a whole that they are willing to cause irreparable damage. I believe that what we can look forward to in the development of the social system is such interdependence of the various parts--in relatively small groups, in nations, and possibly in the relations of nations to one another--that a cooperative system would be more likely to be the ultimate result than anything in the nature of an autocracy. And if in the evolution of society a cooperative system should gradually develop, we should have, it seems to me, a closer and closer analogy of what is going on in our bodies. That is a far cry from an autocratic or totalitarian system, which has all the possibilities of cruelty and tyranny and the suppression of individual interest and effort.

DR. BLAIDSELL: Do the biological and physiological sciences afford us a basis for believing that that kind of a cooperative international society is in the making?

DR. CANNON: Not biological science, and not international cooperation. But there is evidence of cooperation within national boundaries. About four years ago I was in Finland and Sweden and Norway. While in Stockholm I had the great privilege of spending a morning with Mr. Hedburg who showed me the workings of the cooperative system there. It was a revelation to me to see the possibilities of securing coordinated activity in large groups of the population. This coordinated activity was for mutual advantage on the economic side, with a high degree of individual freedom for attention to the things that are worth while and of permanent value. I was very deeply impressed by what I saw there in the operations of cooperative enterprise.

I think you can look back over the evolution of society and see indications of evolution in directions analogous to the directions that organisms have taken in their evolution. In primitive societies some groups lived by the chase, by killing their prey, and they were obviously much limited. There was a low degree of freedom, for they depended entirely on getting enough food animals to provide them with nourishment, and that might vary greatly with the seasons. Other forms of primitive society were agricultural. The agricultural groups depended largely on whether there was a return for the efforts that they made in securing food from the earth. As society has become more complicated and there has been a larger degree of organization; the possibilities of securing food, for in-

stance, have been rendered more and more certain throughout the year by preservation in storage.* Now, that is what happens in the organism. We have occasional intake of food and constant need for using it, and the adjustment between the occasional supply and the continuous use is made by storage. Society has become gradually much more complex than it was in primitive times, and in the course of that evolution it has become more and more freed from the disturbances of nature or the oscillations of the seasons by adjustments very much like those that have appeared in the bodily organization. I think, as I pointed out yesterday, it is somewhat dangerous to draw close analogies** between nature in biological organization and our social organization, but we can regard nature as speaking in parables. Here is a parable that seems to have at least suggestive significance.

DR. CHAPLINE: I think one of our difficulties may be in the development of society. We may expect changes to occur too promptly. You mentioned yesterday, I believe, that when a situation in the body gets out of order there is a contrasting effort to bring it back into place, and that we have come to recognize those things as more or less automatic. Well, in our social developments, if we look back I think we can note that we go through certain spells where conditions get too optimistic and they are brought back to a reasonable basis. We get depressed and then we are brought back. I think that if the average run of person had been told in 1928 or 1929 that he must stop speculating, as practically everybody was doing, and that he must stop that immediately, he probably would have taken exception to it. But excessive speculation came back down on our own heads and ran us into a severe depression. But on the other hand, aren't our trials and tribulations in a social order of the type that bring us great developments? I think that most anyone would agree that the period through which we have been going in the last eight years has probably meant one of the greatest social developments that we have had in a long, long time.

I recall a friend of mine who in his 50s had a heart attack, fainted on the street and was taken to the hospital. He had a few days for reflection, and after that I suppose watched himself closely because he lived considerably beyond twenty years after that period, and when past seventy he took a trip around the world and drove back and forth over the continent several times, and lived a very active life. The fact that the social order does go through its depressions doesn't necessarily mean that we are going to be down there all the time. If we again go back and review what has happened, we see that while there is a certain amount of regimentation that has come about, it is the natural reaction; we must accept a certain amount of that; but still there is in the democratic process a development of a greater appreciation for social betterment. And we have had a big reaction in that way from the depression. I think there is a very close analogy between the evolution of the body biologic and the democratic process, if we

* Dr. Levy carried this over to an analogy with science; see section VI of his lecture. ** P. 202.

don't expect too rapid changes. When you look at Germany you will see that the government there has decided that certain things should be done for the general public, and most of the people there, I think, believe that it has been to their advantage. Possibly it is, but they have in that process lost a lot of their own individual faith, belief, and courage. Here we haven't had to lose our individuality, and I think that we shall come out of it and that we are coming out of it, in fact have already come a long way.

DR. CANNON: The point that you made regarding the value of a disaster is interesting because of its relation to the development of regulatory processes in society. I should say that what the last eight years have shown is that these extreme shifts of economic conditions are so serious, so harmful, so cruel in many ways, that we must set to work to devise means of avoiding them; and those means, it seems to me, must be directed in ways that are analogous to the arrangements that prevail in our body biologic. There is more control over the economic situation now than there was eight years ago. There is provision for reserves of one sort or another. There is provision for continuous activity of labor. There is a control of processes that are disturbing the stream of commerce that ten or fifteen years ago would have been regarded as appalling, and these innovations have been the consequences of the sad experiences of 1929 and the years that followed. They are intended to act in the direction of minimizing the disastrous ups and downs of the economic situation. As intelligent units in a social organism we are able to bring about alterations and improve our methods of control much more rapidly than would be possible in the natural course of evolution of unintelligent units in the body.

I am afraid I am not making my meaning quite clear, but I am trying to say that although there has been a black period of depression, it has been leading to the sort of control that the body has learned through centuries of experience.

There is one point that I should like especially to emphasize in regard to controls. In the body biologic there are warning signals that indicate at a very early stage when the possibilities of danger arise. We have a temperature regulator, a thermostat at the base of the brain. It operates when there is a tendency for the temperature to fall. Then, as I pointed out yesterday, there is a contraction of the surface vessels so that the continuous stream of heat leaving the body is checked, and heat is conserved in order to maintain the even temperature of the body. If that doesn't work there is a second line of defense, a liberation of adrenaline which speeds up the burning process, as when a blower is put on a fire. And if that isn't sufficient there is a third line of defense--shivering, automatic muscular contractions that produce a lot of extra heat. These are all ways of preserving a constant state of temperature. When a dangerous economic situation was developing, as in the late 20s, there ought to have been some economic signal, it seems to me, that would have allowed the application of economic controls, superior to merely telling people, "You had better not speculate." Might there not have been operations of the banking system that would have put a check on the disastrous development? If we had had enough experience,

enough intelligence, to apply methods that might have been applied, if we had had warning signals might we not have avoided the catastrophe? There are persons who said that those signals were evident and who profited by knowing it; and there were others who didn't. I think one of the most important things that economists and sociologists could do would be to study these big swings of the economic conditions and learn where the warning signals might be looked for. This is out of my line; I hesitate to say more.

DR. CHAPLINE: There were certain warning signals apparently available in connection with the 1927-1929 excessive speculation. Apparently we went through a comparable depression in the nineties. The country came out of that without the loss of its democratic principles. Even though there were a good many warnings about the Florida boom, which as I recall happened in 1927, few people would believe in them, and many people went from all over the country to Florida to help get in on the flop which came along about the time they got there. I remember being in Mancos, Colorado, a little town in the southwestern corner, on a narrow-gage railroad. It was in 1929 and I was waiting for the train to come in. The driver of the dray, the station agent, and another man whom I couldn't recognize, were discussing their speculations on the New York stock market, all ready to be plucked in good shape. There were lots of warnings given, but apparently we were unable to recognize them. But out of all of that has come the sort of thing that you have mentioned as a result of our democratic processes. We have put checks now on a lot of those things that we hope will prevent a similar depression in the next generation.

DR. CANNON: That is to be hoped for.

DR. SCHAEFFER: Dr. Cannon, you mentioned intelligence just a few minutes ago, and I was wondering if this intelligence was concentrated in just a few cells, or is it distributed all over the body, or where is it?

DR. CANNON: The part of the body that is concerned with adjustments to the world about us, to the external environment, is the cerebral cortex--that is, the outside of the large part of the brain--and the connections that it has with the sense organs and the voluntary muscles. These are the mechanisms by which we learn about our surroundings and adjust ourselves to them, modify our relations to our surroundings and modify the surroundings themselves; make structures like this building, fly aeroplanes, run ocean liners, push baby carriages, and do all sorts of things that we have to do in relating ourselves with the outer world.

MR. JACKSON: Isn't our inventiveness making these surroundings more complicated all the time; new gadgets, new regulations, new economic planning?

DR. CANNON: Undoubtedly our external environment is becoming more and more complicated all the time because of new inventions and new applications of science, but I do not see that there is any way by which that

environment can become more complex than the ability of the brain to make adjustments to it.

DR. BELL: It might be a fair comparison in a democratic system if we likened the external sense organs to the county committees and other people that are gathering information as individuals and pouring it into the Department of Agriculture with the hope that we shall say, "We shall be able to coordinate that information and reach a conclusion that might be a workable one." We all know that the brain itself doesn't have all the intelligence in one spot. We all know it is distributed through the cortex, and the inflow of stimuli comes from a great variety of sources. It seems to me that there in a way is a picture of a democratic society. All these agencies are pouring in information. If we are alert, we keep the channels open so that the information will reach the right coordination center--it might be the Department of Agriculture, or it might be the President or some of his working committees, or what not.

DR. SCHAEFFER: Dr. Cannon, you mentioned yesterday* that we could get along with one kidney and one lung and less liver. Now, what is the function of the extra kidney, lung, and liver?

DR. CANNON: I regard that situation as supporting the evidence that security is more important than economy. The body, as I pointed out yesterday, is organized in such a way that there is a high degree of assurance of stability. I illustrated a few moments ago the three lines of defense against cold which we have inside us and which operate one after another. The last is shivering. Well, shivering, as I said yesterday, is a waste of energy, but it accomplishes the desired constancy of temperature. The whole bodily organization is built with a wide margin of safety. If we lose a kidney we still go on. We can lose half the lung area and we still go on. There is a great deal to indicate that in our bodies security, stability, constancy are more important than economy.

Question: Dr. Cannon, is there any evidence to indicate that any of the processes of the body which you mentioned last night have undergone change during the period of man's knowledge?

DR. CANNON: Well, the paleontologist can go back to the age of reptiles and show that those organisms, millions of years ago, were built on a wrong plan that didn't work, and they passed out. They had huge bodies and very small brains and they didn't succeed.

QUESTION CONTINUED: And would it be safe to assume that the human body through a long period of evolution has reached a stage of perfection where it now undergoes no further change?

DR. CANNON: I think there is fairly good evidence that the body as such is not undergoing any considerable alteration. It may become somewhat better developed. There are reports that the Japanese have gained in stature by changing the character of their food. Perhaps by a better knowledge

* P. 205.

of vitamins and other food elements we can improve the physique, but I doubt whether we can change the essential bodily structure. Everything seems to be directed toward an improvement of the services which the brain performs--towards observing new relations between things that we didn't see were related before. That is what invention comes to, or what discovery comes to, in the last analysis. In that direction are possibilities of infinite progress.

QUESTION CONTINUED: There are mental processes that are applied to society, man's society, are there not? They are not applied to man's physical function itself in any way.

DR. CANNON: No, except so far as we learn about vitamins and other elements that may help our bodies.

QUESTION CONTINUED: Last night my thoughts were going back to the statement in the Bible that God made man in his own image, and that now apparently man is in the process of consciously setting up a form of society that is in the image of his own body. I was wondering whether that is a fair statement of what you said?

DR. CANNON: I don't think that man is consciously doing it.

REMARK: No, subconsciously doing it.

DR. CANNON: We have been undergoing an evolutionary process and it may be that society is undergoing a similar evolutionary process. If you go back to the Neanderthal man or the Piltdown man, you observe evidence of relatively little change in the skeleton except that part which contains the brain.

DR. BELL: Haven't we a lot of old relics sticking around our bodies like excess growth of hair, peculiar formations on the ears, appendix, and all that sort of thing?

DR. CANNON: There are indications of our remote ancestry, but I don't think there is evidence of any change in the last two thousand years.

DR. BELL: In a short period of time you are perfectly right, but we all know that from the anatomical standpoint, there has been a tremendous turn-over, and there are a lot of old relics sticking around our bodies that are not now essential, but which at one time probably were. Another point comes to my mind from your answer to the question that this gentleman raised about the so-called excess organs. It seems to me that the most inspiring and encouraging thing about the whole thing is that where we have two kidneys that is the normal situation; that is the situation in which things are carried on. But if something goes wrong and one kidney has to be taken out the other kidney takes over the excess load. That is true in society. Something goes wrong, you see that over and over again; yet there is always

something to take up the slack and carry the excess load. In college, of course, we always have the feeling that after the senior class goes out the college is ruined, but the next year another class comes along and takes up where they left off and the college goes on just as well as it did before. And society can be counted on to do very much the same thing.

DR. SCHAEFFER: Dr. Cannon, does the body have any particular group of cells that might function to get rid of surplus commodities?

DR. CANNON: Yes, I didn't emphasize that point yesterday afternoon, but there is a process of maintaining constancy by overflow. We eat too much sugar. It rises above the threshold, the kidney threshold, so-called. It goes up to 200 milligrams per cent in the blood. We will say 170 is the threshold. The excess of sugar will pass out. We get constancy by simply unloading. If there is an excess of salt there is an overflow. When there is excess of water there is an overflow. These are instances of maintenance of a constant state by getting rid of the extra quantity through loss, actual loss.

DR. BLAISDELL: Dr. Cannon, I think we have discovered some semblance of a biological justification for the over-normal granary and for the removal of the price-depressing surpluses, and if we could continue this discussion we might be able to find justification for everything in the Department of Agriculture. That would be a very satisfactory denouement, I am sure, of this discussion, but the time has come to wind it up, and I am sure I am expressing the feeling of the people here that we are very appreciative of your kindness in taking the time to come down yesterday and again this morning to talk with us.

DR. CANNON: I have enjoyed it very much.

Ten Lectures and Discussions on

SCIENCE: Its History, Philosophy, and Relation to Democracy

held at the Department of
Agriculture Graduate School
under the chairmanship of

M. L. Wilson
Under Secretary of Agriculture

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Edited by W. Edwards Deming
Assistant Director of the Graduate School

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Lecture IX

HOW CAN SCIENCE GET SUPPORT IN A DEMOCRACY

by

A. J. Carlson
Professor of Physiology, University of Chicago

The Graduate School
The Department of Agriculture
Washington
1939

Lecture IX

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In the auditorium

I

Mr. Wilson, I am glad to be welcomed again to the Department of Agriculture. I am not entirely a stranger in some parts of your enormous establishment, but two weeks ago when I accepted Dr. Deming's invitation to come here I did it in a moment of the role of the absent-minded professor, because when I found time to look at the subject I didn't see how I could contribute anything worth your time nor mine. At my age the loss of time isn't worth much, but you contribute your actual dollars to my traveling expenses and those dollars are earned, I hope, in the sweat of your brow.

The topic doesn't permit me to speak in the field where I am most at home, but it does permit me to speak on matters to which I have given a good deal of thought during my working years, so it is not entirely unwelcome. Most of you, I dare say, with your background, have ideas of your own on this subject, and I assure you I come here with no dogmatism of my own, except on one or two points, and the first point is this: that I am from experience an incurable democrat, spelled with a small "d"; and the second is this, also based on experience, that science must secure a far broader base of intelligent support in society than it has at present if it is to render its great service and if it is to endure. I have seen too many places in the world where an esoteric science, an Ivory Tower, superimposed on the broad plain of human ignorance and human misery, tumbled and is now rotting in the dust. That is not the kind of future I like to vision for science in this country--in this democracy.

Support for science involves, to my notion, two things; first, support for real scientific education; and second, support for scientific research. I mean by scientific education the kind of education that leads to understanding of the nature of science and the consequences of science. And you may infer, I think, we have not that kind of education now, at least effectively, so that even our high school graduates or even our college and university graduates really understand the essentials of science. They understand the gadgets, they enjoy the gadgets; they may memorize many scientific facts, but that, of course, is not science--the kind of education that would give to every child, every man and woman an understanding of the nature of science, the spirit, the method, the necessary hesitance in expressing or concluding anything until you know all the facts that can be made known, the necessary patience in establishing your controls, and above all, the absolute honesty required in science. All those things have not yet taken root in the common man.

Educators, statesmen, leaders have tried to sell science by its gross fruits, by its gadgets by its consequences. It pays so much, and it is relatively easy to sell the science of agriculture and veterinary medicine to a farmer with a sick hog, or a diseased corn field or wheat field, and it is relatively easy to sell research in medicine to a man or a woman with a headache or a gastric ulcer or a diseased kidney. But I think if we try to sell science to men and women in a democracy, we shall fail if our sales talk is solely on supposed or actual material benefits. I say this on the basis of experience, and I have tried to teach for nigh on to forty years. I believe that the average man and woman in this country, the average boy and girl in high school is capable of conceiving, of getting something more out of science than the thought of immediate material gain. I think people can learn to appreciate science for the sake of a broader understanding, for the sake of eliminating fear, for the joy of solving or having solved or seeing solved the riddles in man and in the universe that we face and that our forefathers faced for a million years. You seldom hear of that element in science. That value of science, though, is one of the greatest; and I believe it is a mistake to take the view that the average man or woman is incapable of seeing it, incapable of appreciating it, and incapable of being appealed to on that basis for its support.

In criticizing our prevailing education in this way, I criticize myself; I am one of the sinners surely. It is far easier to drill facts than principles into your children, your own children (and I have tried to educate children of my own), as well as into your students. It is far more difficult and far more expensive to have the children learn by doing; to introduce a little of the experimental methods into the natural sciences in the high school, and above all to teach science, as it really can be taught, by the example of what we ourselves are doing.

I went to a small, financially poor college nearly fifty years ago. In a humdrum way I read books; I was omniverous. I read Greek and Latin and French and German and English and rhetoric and philosophy and psychology and chemistry and physics and biology. As a matter of fact, in my junior year I exchanged Greek for petrology and I soon got tired of both. But the man who really opened a new world to me was a professor in the college who was doing experiments in a very inexpensive way on the storms and wind, the movements of the atmosphere and the changing configuration of the earth. He had some small boxes at different levels on a high pole on a hill, and he accurately weighed and measured the dust particles collected in those boxes from time to time. He invited me to help him do it, so I found out what he was driving at, and--according to William James' old idea--whatever unused or untouched lakelets of energy there might have been in my brain certainly were tapped, not by anything that filtered down into them in class but by what this teacher did. And if we could have a little more of this done in the high school, instead of in many cases having state laws actually preventing experiments of that type, particularly in biology, we

would get along better. But the sinning is not confined to the high school; it is also in the college and in the university.

Now that kind of education, it seems to me, is essential for action in a democracy, because by action in democracy I understand an action based on the understanding of the substantial majority, and acquiescence without violence by the minority.

If we look back a minute on the beginnings of science, so far as we have any record, we see that man's early interest in that field started by contemplating the heavens and by the urgent drives of his personal pains; that is, it started in astronomy and in medicine. So far as medicine goes, to judge by the Irving Smith papyrus, translated by that brilliant Egyptologist, the late Professor Breasted, that is the earliest human record in science. I have read the document, and whoever they were, those unknown doctors of about 5000 to 7000 B. C., they did begin to use the scientific method in diagnosing human injuries and diseases, in contrast with the prevailing sorcery.

A few years ago I had the privilege of visiting parts of China. At that time, in one of the corners of the walls surrounding Peiping were still the old astronomical instruments. How old they were I don't know. In China, apparently interest had started in astronomy and advanced farther in that field than in the study of disease. I learned that vaccination against smallpox had been practiced in China many hundred years prior to Dr. Edward Jenner and the milkmaid in Great Britain. The Chinese did it by taking flakes of the scab of a convalescing patient and rubbing that into the nasal mucosa of a well person. Vaccination against smallpox can be made in that way.

II

Support of science means, then, an understanding of its nature and consequences; and, secondly, financial support both for education in science and for research in science. Now what is the past history of such support? In our country, of course, from early colonial days, we have had a halting but gradually progressing democracy; and support for education, including education in science, started early through private local initiative, and thus we have such institutions as Harvard and Princeton and Pennsylvania, which started as a private institution. Parallel with that, there early started state support for state universities and state colleges; and very early--as this group knows better than I--there started federal support, particularly for education and research in agriculture, partly centralized in this city but more largely spread out into the forty-eight states, in the state agricultural colleges. I need not mention, of course, the other type of federal support of research in the Bureau of Standards, in the Public Health Service, in the Biological Survey, and other similar endeavors So, during the past, irrespective of what the understanding has been or the failure to understand the spirit, the nature, or even some of the conse-

quences of science, there has been fair support of science by individuals of means, by local communities, by states, and by the federal government.

Now my subject raises the question, do we need more of this from any source--private, city, state, or the federal government? Is private support drying up? More recently there has come support of scientific research by industry, directly in industrical establishments. That research is largely, of course, determined by the needs of the particular industry in improving its products, in finding new products, in getting ahead of its competitors, and so forth. And while some of the research support in industry and in industrial laboratories is devoted to fundamentals, while some of the workers who are given their heads (as they must be given, if there is to be fundamental research), by and large, I think, science in our country would be in a bad way, if we were entirely dependent on the kind of support that industry gives in its own research laboratories.

Of course there are exceptions. Look at the Bell Telephone Laboratories. And many industries interested in the new medical remedies, new drugs, better drugs, less poisonous drugs, to control human and animal ills or even plant ills, frequently do respectable experimentations. But I certainly would consider it perilous if our state universities, our independent universities, and our research departments in the federal government should be forced to slow up instead of permitted to increase their speed.

One practical problem comes up. I don't know whether Mr. Wilson and the other leaders had that in mind or not, but I absolve him and the rest; they didn't tell me what to say, and I can tell you now it wouldn't have done any good anyway. One of the problems that faces this country now is the drying up of private financial aid to our independent colleges and universities, not intended I am sure by anyone, but a development of the government's attempt to meet the financial calamities of the nation during the last few years. Permit me to quote from an article published in March of this year by President Wriston of Brown University.* It is very recent and I think it is probably accurate. I quote:

> In the year 1928, just a little over ten years ago, there were nearly 200 gifts to American colleges and universities of over $100,000 each. Last year--that is 1938--that number was less than fifty. There was a similar decline in smaller gifts.

You see, the interest of private individuals in such institutions as Harvard, Princeton, Johns Hopkins, Chicago, and others too numerous to mention, has been their only lifeblood. I quote again from President Wriston:

* Bulletin of the Association of American Colleges, March 1939 (19 West 44th Street, New York.)

> Furthermore there has been a significant capital shrinkage in endowments since 1928, and the yield on sound investments is much less than it was in 1928. Moreover, the decline in return on investment is not at an end, because with everything the Government does, including the expansion of social security, it is promising no end to deficit financing. So long as we have deficit financing the Government must maintain cheap money rates. It will continue to be regarded, indeed it must be regarded so, as long as we have deficit financing. This makes it evident that the colleges will have largely decreased revenue for some time to come. Meanwhile the student bodies have been growing and the more students the more deficit, because no one charges tuition to cover the cost of training in any field.

So for these so-called private (I call them independent) institutions of higher learning, colleges, and universities, whereas formerly it was a question to have and to gain, now it is not even a question to have and to hold--it is a problem to have and to lose.

I am not criticizing the national situation. I am merely recognizing it. If the contributions to the training in science, if actual contributions to research in science are to continue from these old sources in our democracy, a remedy for the present predicament must be found.

III

I don't know whether you feel like I do about the progress of science in the United States during the last forty years. Maybe I am more impressed by it in my immediate field, biology and medicine. Certainly forty years ago this country was a second-rater so far as research, so far as contributions, so far as the advancement of knowledge in biology and medicine is concerned. We were led by Germany, we were led by France, we were led by Great Britain. And what has happened during this last generation? In biology and medicine we are not only well up in front, but if it weren't bragging, we should be justified in saying that in some fields we are pretty near in the lead. And that has come by a combination of forces, but largely from the efforts of individuals in our private institutions of the higher learning. I am not unmindful of the contributions to the science of agriculture and in veterinary medicine by the state universities and by the federal government, and the contributions to medicine, some of them very great, by the Public Health Service, in the field and here in Washington. I know all that. Nevertheless, if older sources for support of science are blighted, we are going to slip and slip rapidly unless other sources of support are found.

I can't quite explain the reason for the tremendous resurgence in this country of scientific work, research in biology and medicine, during the last generation. Certainly we have no better brains now than fifty, one hundred, or one hundred and fifty years ago; human mental capacity doesn't change that fast. Maybe there were no more railroads to build across the country, no more canals to be built, and human attention was directed in other fields. Maybe we got some of our stimulus from abroad. Maybe we felt a challenge. Maybe our pride was stirred. Whatever it was, we began to tap one of our great natural resources, namely our mental capacity. Research in science for the enjoyment of man has not only added many new and strange gadgets of living, but has greatly enriched our intellectual life, to the satisfaction to the individual, to the group, and to the nation.

IV

In the little time that I had to think over the subject assigned to me, President Hutchins called my attention to what the British Government started to do for the universities and colleges in Great Britain and for the hospitals and for hospital research, medical research in Great Britain, following the World War. I have here a report of that work in Great Britain up until 1935, from the University Grants Committee.* This committee has handled grants from the Imperial Treasury to these institutions, amounting to about 11 million dollars a year. It is handled by a committee of experts, not connected with any one of the institutions, by people who know the nature and the needs of education and research. It is only fair to say that the use of these grants is much wider in England than I have in mind for our country. In Great Britain the grants are even used for building dormitories. But the technique is something that is worth studying by individuals in government, and others who are concerned with the present danger to our independent beacons of light. The men appointed on the University Grants Committee serve without pay, so it would be no source of comfort for our own "lame ducks". They have only one paid full-time secretary. I should like to quote two or three sentences of the report, because I think they apply to institutions in the United States. From page 59 I quote:

> "We believe that the British universities may be trusted to make the best use of any new resources which may be made available to them."

I think that statement could be made, without many exceptions, to the private institutions of higher learning in this country, and I think I know most of them. You probably will not thank me for suggesting that something like this might be done in the United States. I don't know

* University Grants Committee, Report for the period 1929-30 to 1934-35 (His Majesty's Stationery Office, London, 1936). Price 4/-.

whether it should be done or not but I do know this--I think I know this--that before any such scheme on the great scale is started it should be thoroughly studied, it should be thoroughly discussed, it should be made so clear that it has understanding and support in society and will not shift up and down with our sudden political whirlwinds in the state or the nation. Moreover, I should want to see the machinery of organization and control worked out so that other sources will not dry up, the private and local sources that are still available to such institutions. That would be a calamity.

I certainly would not favor any 700 million or 800 million support for general education until such a system has been worked out. But there is one small way in which support could be given to science and scientific research with our present existing machinery and without any danger whatever. We realize that in the past, the training of scientific workers for society and for the government has fallen largely on these private institutions, including those supported by taxation in the states. To be sure, Michelson, the great Michelson, came from our Naval Academy, but our Naval Academy has not given the nation many Michelsons in science. To be sure, in the army and in the navy schools some training in engineering is provided; but because of their very nature our army and navy academies are not great training schools in any field of science outside of engineering and I am not aware that they have added materially to such trained personnel.

Our federal government does contribute to the training of experts in various fields of agriculture, through its original land grants to the forty-eight states and through its continued support of these institutions. That is perfectly true, but in pure science, physics, chemistry, biology, astronomy medicine, mathematics, there is little or no such support. It would be worth trying, I think, to the amount of the cost of one or two battleships, to see what federal grants to graduate or postgraduate research fellowships would do in this country. We have the machinery for it. I think if there is any national institution that has come out with flying colors in administering national fellowships, it is the National Research Council in the natural sciences. I know less of the work of the Social Science Research Council, but the former has scoured high, and I can say so without prejudice because I have had very little to do with it personally. Such funds from the national treasury could be administered like the fellowship funds from the Rockefellew Foundation, which, by the way, is one of the other things that is drying up because of the reduced income of a number of endowments; the fellowships are smaller and they are fewer in number.

V

I suppose all of you have received and have studied the report of the National Resources Committee, Research--a National Resource vol. 1. "The relation of the federal government to research" (Washington,1938). I don't think much of the English there. Research is a process, hardly a national resource, but the development of our mental capacity and understanding through research is the development of a national resource. On page 4 they give a number of

recommendations. Some of them refer to the improvement in the status of the research personnel in our federal government. But one of the recommendations is this:

> That research agencies of the government be authorized and encouraged to enter into contracts for the prosecution of research projects with the National Academy of Sciences, the National Research Council, the Social Science Research Council, the American Council on Education, the American Council of Learned Societies, and other recognized research agencies.

That, I believe, is an unwise recommendation. These are not research institutions. The matter should be carefully considered, and some such plan of operation as that of the University Grants Committee in Great Britain would in my judgment be preferable, although I have a great deal of respect for the members of the committee who are responsible for this report.

VI

One of the early interests of men in the beginning of science was medicine. The federal government, state governments, city governments, private individuals, have aided research in medicine throughout the ages. I wasn't particularly enthusiastic over our recent program of extension of Public Health, because most of the funds were to be allocated to service rather than research, but we have now a beginning experiment of what may be done, in the development of the Cancer Research Institute as part of the Public Health Institute in these United States.

By and large, in the past, it has been easier to obtain private funds for needed research in medicine than in almost any other field. But now, with the private resources drying up, it is just as difficult for the medical schools and for the research hospitals to carry on as it is for the universities in other fields. During the last three or four years I have visited numerous state universities, state colleges, city colleges, in nearly every state in the Union. I have been struck by many things on these campuses but particularly by one thing, the enormous building programs--laboratories, lecture halls, dormitories and hospitals, all over the map, on federal loans, federal gifts, and state money. I have the impression that these institutions are now developing us into the stone age. Buildings are not research. Buildings do not make a university. Buildings are not education. I am not criticizing this development. It is probably a necessary part of providing work for fellow citizens who are unemployed and without food. I am not criticizing it, I am merely pointing it out. It is going to be a sad day for us if the wherewithal for men with character and brains, if the means and the freedom to use these new halls as they should be used, if provisions on local and national scale for the continued and increasing support of fundamental scientific research and education are not found speedily. This is a national concern. We should have a program, not subject to the vicissitudes of political parties, or the life span of individuals. Where is the Cato of our day, who will insistently remind us all: <u>In a democracy, science must be supported by all the people</u>!

DISCUSSION

following the lecture by
Dr. A. J. Carlson
Professor of Physiology, University of Chicago

Editor's note: Dr. Carlson could not stay over for the seminar and therefore a few questions were sent to him which he kindly answered by mail.

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MR. GLICK: We hear frequent reports from Germany, Italy, and Japan of large sums of money devoted by the governments of those countries for research in chemical warfare, airplane construction, development of "ersatz" material, etc. On the other hand, we know of the denial of freedom of opinion and inquiry in these countries. This suggests the following question: Is science more likely to thrive under "democracy" or under "totalitarianism?" Further, what do you think of the suggestion that certain types of scientific research--such as those involved in preparing a country for effective warfare--are more likely to thrive under "totalitarianism" than under "democracy?'

DR. CARLSON: The lessons from history, as well as the very nature of science, seem to furnish the answer to your first question: Is science more likely to thrive under democracy or under totalitarianism? Certainly in the past, as I read history, the essential contributions to science have been made by relatively free men, applying their intelligence in seeking the answer to their curiosity rather than by dictates from rulers or bosses. Any authoritative restraint appears to me an unfavorable environment for the workings of the scientific mind. I know there are a few of my colleagues who take the opposite view. In a little book published a few years ago by Dr. Muller, entitled Out of the Dark, the author makes the categorical statement that the totalitarian state, as exemplified by soviet Russia, is the only political organization of a nation favorable to the progress of science. Dr. Muller's little book should have been entitled not Out of the Dark, but Into the Fog. History is against his dictum.

Yes, certain types of research of immediate need in preparation of tools and gadgets for war can, undoubtedly, be speeded up under dictatorships saturated with war mania. But I am less interested in that type of research than in the pursuit of the problems whose solution will throw additional light on the nature of the universe and the nature of man. The force of immediate needs of war, and government urgency and support of such research may not be so successful, even in apparently and relatively simple and practical problems. For example, in the late World War the governments of Great Britain, France, and the United States were greatly concerned by the high percentage of fatalities from traumatic shock in wounded soldiers and, stimulated by

these governments, medical men set to work on the problem of the nature and the control of traumatic shock. A great deal of work was done in the laboratories and on the fields of battle. Some of the work was useful, but in my judgment, work of much greater significance and promise on the nature and prevention of traumatic shock has been done by individual scientists in peace time after the war than was done as the result of war needs and war pressure.

MR. GLICK: I assume that science needs "support" in a democracy in a broader sense than merely financial support. When people see the results of science producing problems and disorders rather than progress and power, they may lose admiration and respect for science. This suggests the following question: How can dependable scientific knowledge be brought to bear on the social and political problems of our day? Or, to state this question in another way, what is the relation between "science" and the socio-economic order? To state this question in still another way, can scientists do anything, as scientists, about the tendency for its results to be used, frequently, for destruction rather than for creation and progress? What?

DR. CARLSON: Scientists are just well trained citizens. If united on a proposition, they should be able, as citizens, to aid society in working out solutions of practical problems better than those not trained in science. But the use of the fruits of science for purposes of war or destruction rather than for the broad welfare of society is not so much a matter of intelligence as of ignorance, emotions, greed, and fear. The only cure for this that I can see is better adult education, and this terrific burden can not be carried on the shoulders of the scientist alone. The various ills of our social-economic order are partly a matter of tradition, partly a matter of human selfishness, and partly a matter of ignorance. As I see it, true scientific education and true scientific research will be of material assistance in slowly changing man's emotion and man's greed, but science thus applied is not a cure-all, effective over night, or in a single generation. It is a matter of the long pull, persistence, and patience.

MR. GLICK: What should the government do about scientific research? Encourage it? If so, how? Leave it alone? If the government supports scientific research financially, what is the danger; will this lead to limitations upon the freedom of scientific inquiry?

DR. CARLSON: Yes, government financial support of scientific research, if carried out by the methods and techniques of myopic bureaucracy can and undoubtedly will lead to limitations on the freedom of scientific inquiry. Autocracy is foreign to scientific inquiry, and the fullest individual freedom is a necessity for the individual investigator, if in the long run the best results are to be obtained. I think it is an axiom that with man, as we know him at present, autocratic power, whether in politics or in science, can be trusted with but few people for short periods and with no people in the long run. I think Great Britain has shown one way in which government may support independent research institutions without the danger

of dictation by myopic governmental bureaus. I believe the machinery used by our National Research Council in allocating financial support available through one of our large foundations for research on sex; of allocating such support to the institutions and individuals engaged in this type of research, and best qualified to carry such research through, could be applied without great difficulty to government support of research outside of the direct research arms of such government.

DR. DEMING: Dr. Carlson, I have been interested in the British plan of providing grants to the universities for the support of research. Your suggestion that we might well think seriously of formulating a similar plan for this country has far-reaching possibilities. You spoke highly of the National Research Council and suggested that it could be trusted to administer funds from the public treasury if any plan such as British Grants Committee were put into operation (p. 226 of your lecture). Then later (p. 228) you questioned seriously the recommendation of the National Resources Committee. I should like to understznd your point of view and what you would put in place of the recommendation of the National Resources Committee.

DR. CARLSON: Maybe I did not make my point clear. There does seem to be a contradiction in my apparent approval of the British University Grants plan and my questioning of the wisdom of contracts for research on the part of our own government with such bodies as the National Academy of Sciences and the National Research Council. Let me try to clarify this point. The government support of research in Great Britain is direct to the universities and hospitals under the guidance of the Committee of University Grants. These are, in part at least, the places where the actual research is done. Now, the National Academy of Sciences and the National Research Councils are advisers to our federal government on research as well as on other problems; and I do hope that in the future our federal government will make more use of these bodies in an advisory capacity than it has in the past. As to this advisory service, these bodies can show a good record, both as to intelligence and social conscience. But these bodies are not research institutions, or at least they are so only in a limited way. They are aggregations of scientific men, in most cases still connected with universities. At most these bodies should be made advisers to the government in the distribution of financial aids to research; that is, to those institutions that are actually doing the research. But I think it would be still wiser to have the question as to the machinery or agencies for federal financial aid re-studied, because I am afraid of the well-known political technique of wire pulling and log rolling, should substantial amounts of federal funds be available for the support of research in the universities and colleges of the land. I should not like to see the National Academy of Sciences or the National Research Council made virtual "holding companies," dispensing public monies to the colleges and the universities.

DR. DEMING: What changes in our educational or political system would you recommend in order that our national and state legislatures could adequately handle questions regarding the disposition of funds for research in

the government departments, experiment stations, and state universities?

DR. CARLSON: I am not sufficiently informed as to details of present legal restrictions. Certainly there can be no legal restrictions regarding government support of its own research departments and the extension of them in the so-called state agricultural experiment stations, that is, the land grant colleges. So far as I know there are no legal restrictions that would prevent federal support of research in the state universities, any more than there is against such support on the part of the legislatures of the several states. But as I have tried to pointout, if we once embark on a program of federal financial support of science in the sense of education and of research, such support should not be confined to the present departments of government and to state institutions, but should be distributed to all bona fide education in science and research in science in the nation. The legal part of this should offer few difficulties. The best machinery for the implementation of such program is the problem before us. The ablest minds of the country must sit in in solving this problem, which essentially is one of making merit alone count, with as much freedom from political interference in the education and research in science as is possible in the present world.

DR. DEMING: Do you think that the various scientific press releases and popular scientific magazines could accomplish anything in the education of the public along the lines of scientific inquiry?

DR. CARLSON: Yes, scientific press releases and popular scientific magazines could be more effective in the promotion of adult education in science than they are now, if they were more accurate and would not sacrifice truth for the sensational, the dramatic, and the miraculous.

MR. WILSON: It seems to me that there are two aspects to this thing of support for science in democracy. One is the aspect of financing science and I agree with Dr. Carlson that with the competition of these new social demands of the state, social security, old age, and all those kinds of things, it is hard for education and science; but it seems to me there is another problem at a lower level, and that is the education on the part of the great mass of the public for an appreciation of science and what science is, and for its cultural aspects. If an appreciation of science by the common man, so to speak, were widespread, then that appreciation would reflect itself in the attitude of public financing for science. And that raises in my mind the question of how to get the general mass of people interested and appreciative of science.

In that connection I think the Science News Service is one of the most important scientific enterprises of today. I feel that there must be a lot of readers, general readers like myself, that read the science news releases in the newspapers, and that they really are or could be instrumental in creating an understanding attitude in the great mass of the body politic, which is necessary to have in order to get support for science especially if sci-

ence will have to depend more on the government.

DR. THONE: I think you are right about that. I think the movement is growing rather encouragingly. I don't know that it is doing as well as it could. Of course it never does as well as it should in our notions, but I have seen some rather encouraging signs just recently. One is the way these little science digest magazines seem to be going. Three or four of them are relatively new and they carry some pretty good articles in them. The science books, popular science books, with, I suppose, some of the technical ones outsell fiction. I really hadn't expected that much. Of course, Science Service is always inclined to take a certain amount of glory for the use of science in the newspapers, because before we entered the field, science was badly neglected in the newspapers. You remember as well as I the standard proceedings....If a science meeting came to town they always sent a cub reporter, the rawest and greenest reporter on the job, and he looked around, picked up a program, selected the title with the longest and most incomprehensible words, and proceeded to write a josh story about it. Now, however, the josh story, if one does appear, is laughed down by the professional science writers. There is a group of professional science writers now, a dozen or so, that follow all the various science meetings; and the local newspaper, instead of assigning the man they can spare easiest, are very much inclined to send one of the best men.

My one criticism of the science writers in general is that most of them do not have formal training in science. The deliberate and announced attitude of the Associated Press, for example, is that if you have a professionally trained man, he writes over the heads of his readers. I think Science Service today proves that to be false, but the newspapers stick to that thesis just the same. Of course they are daily hedging, because the men they have assigned to the job are highly intelligent, and the more they stay on the job the more they know about science.

MR. GLICK: I wonder if we could have a brief statement of how Science Service is organized and just what it does.

DR. THONE: Well, I shall need about three-quarters of an hour to do it properly. I should have to go back to the beginning. It goes back to E. W. Scripps and his friend William E. Ritter, who was head of the Zoology Department of the University of California. The two became very great friends. Ritter became one of Scripps' principal advisers in the philanthropies that he indulged in, as rich men did when they thought they were rich. Now that they think they are poor they don't do it so much. This all happened before the War. Scripps was rather feeling his way around. The War, with the tremendous contribution of science and technology, immediately crystallized something in his mind. Afterwards he said to Ritter, "Science certainly is going to play a very great part in the life of civilization in the future in peace as well as in war. Since it is going to do so, it is important that the people in this democracy know something about this great source of energy, this great source of power that they are wielding, that they have

wielded heretofore blindly or under the leadership of people just as blind as they and sometimes selfish to boot."

So he proposed to organize the thing that eventually grew into Science Service. He put up a certain amount of money, and even yet we are getting an annuity from his estate. So that was the launching of Science Service.

Primarily we were to supply daily mimeographed news stories about science to the newspapers, and we still regard that as our principal function. Then once a week we send out an illustrated full-page sheet, and there are also other services, but far and away the most important thing we do in our own estimation is the service to the newspapers. Second comes the Science News Letter, which circulates now to about 38,000 people. We had no idea of starting a magazine. A few people, only a dozen or twenty, wanted to get those mimeographed sheets, so once a week we bundled them up and mailed them out to them in letter form, and that is how it started, and that is why it was called Science News Letter. As a matter of fact, we deliberately chose an unattractive name, trying to suppress it. It grew anyhow.

MR. WILSON: How do you get your dope?

DR. THONE: Well, we come over here and ask questions, or telephone over, and do the same thing to everybody else everywhere else. We examine no end of journals, reprints, abstracts. Mountainous piles of paper come across our desk. We send people out to scientific meetings. We have a number of correspondents who write for us on space rates, quite a number in this country and a few abroad.

DR. BLAISDELL: It seems to me that you would have a great deal of difficulty trying to keep up with the developments and trends in scientific research, because from the very nature of the subject, those are highly tentative. I imagine you see what I am getting at. You aim to publicize the results of science in order that they can be more widely disseminated among the common people. Isn't there a real danger that in publicizing those results there will be some mistrust and misgiving?

DR. THONE: Yes, there is always that danger. Of course, being human, we frequently fall into that trap, but we have learned that there are at least a few generalizations to look out for. We used to get out every two or three years a sort of a lookout for this list, which we would send out to our editors, stories to be careful of, the kind of stories that we ourselves rarely or never publish, that we caution the editors against using--perpetual motion, cancer cures, voyages to the stars, fantastic things like that; and then other things that are more plausible, such as the discovery of a giant extinct race. Every time someone finds a skeleton that was buried in the supine position and the feet flop down, they measure from the toes to crown, and a man of my height might very easily measure nine feet when they got through with him.

DR. TUCKERMAN: I was guilty of "newspaperitis" once. One of the best

stories I ever turned over to them was when this light concrete appeared in Sweden. You blew it up by putting aluminum in it. I called up Watson Davis and said, "raised biscuit of concrete, not so light as mother used to make, but twice as durable." That was the caption. Now the technical facts were there, you see, and that went all around the world because we had the right sort of newspaper caption. The Engineering News Record cussed us out for making a newspaper story of that, but here was a purely technical piece of work, you see, that was blown up this way and made to look funny. The information went around the world. It was quoted in Australia. The point I criticize is that the magazines get so much material that when they get a nice little newspaper caption they are not so careful about the facts.

MR. WILSON: I wonder if in our field in agriculture we are missing something by directing so much of our publicity and information on the applied level and not stimulating enough appreciation of the fundamental.

DR. THONE: I think it would help some if some of your releases gave a little more of that. Taking what I regard as three specific examples, the last three yearbooks, they are beautifully brilliant, and get a general concensus of the whole field, even if only symposia. They are the most highly successful biology books that I know. I think possibly if some of those things were developed as news articles that they would have a good chance of being used.

DR. TUCKERMAN: I have had this come up. Here is a problem that we can't solve and we have tried to persuade people that we should have a backing for something that we don't see, and which promises no immediate usefulness. Can I get money for it?

DR. WOODS: I have been going up before appropriation committees for this Department for close to forty years and I never have found a committee that as a whole wasn't favorable to fundamental research, whether we could show any immediate returns or not. In all of my experience, any sound piece of research has always had their support, and I have known them to make appropirations for such types of research even against the recommendation of the Bureau of the Budget and the President.

DR. KELLOGG: Good examples of that came up from time to time. I have seen instances in the past of the House and Senate Committee adding items to the agricultural bill for fundamental research that had been thrown out by other people along the line before the bill even got to the committee. There is a problem of selling fundamental research in the Department of Agriculture long before you ever get to the point of the Congressional Committee.

MR. WILSON: Most of our reports of experiments and that kind of thing are not wholly but to a certain extent dealing with the past. Now, shouldn't we play up to the public the future as well? Here are problems arising or developing. Why couldn't we express the hope that science could do something in connection with them?

Dr. Stockberger, you have seen this thing for a long time; what is your comment?

DR. STOCKBERGER: I think the history of the Department, so far as I know it, when looked at in the large, will show that while there may have been on certain occasions some evidences of attempts at control, by and large there has been a great degree of freedom in the research work which has been carried on, and I think that the same principle applies to the government supported institutions, particularly the land grant colleges. The trouble that I see in the land grant colleges is not so much domination by censorship on the part of the government, but yielding to too much popular sentiment, or belief in popular sentiment, with regard to their activities. I cite as an example the fact that land grant colleges have pretty generally now dropped the study of any language except English.

DR. WOODS: In many institutions the men engaged in teaching have no time for research. Nebraska has just taken the step, in the unicameral legislature, authorizing the university to limit its student enrollment to the point where it can be effectively handled with the money available. They can cut down to 2500 if they want to, even though 5000 may apply. We have never before felt authorized to do that. We have felt that every applicant who wants to come into the land grant institution must be accepted if he meets the fundamental requirements. This is the first legislative authority that has been granted to give the right to a faculty in a state university to limit the enrollment to the number that they can profitably handle.

DR. TUCKERMAN: I hope they do it. There are several reasons why I left Nebraska, but one was that they thought I should double the hours I spent teaching. I really felt I wanted a little time to think. Maybe I was foolish.

MR. GLICK: The subtle influence of the research worker choosing one line and rejecting another line because one will invite funds and the other will not, is present also in independent and private research institutions, because they know that the administrators of private funds will make their grants for research, bearing that kind of research in mind.

DR. TUCKERMAN: "Man shall not live by bread alone . . ." After all the direct contributions that science can make to civilization, at least if we exclude psychology and sociology, lie chiefly in the field of "bread." I do not agree with those who say the contributions of science have made the problems of society worse, but the notable contributions of science to the world in the way of "bread" have, as we all know, done little if anything to settle the age-old problems of a satisfactory social organization. Nor can man live by reason alone. Science, the embodiment of reason says "if you do certain things, certain things will happen." Technology says, "I want to do this sort of thing" and asks science "How shall I do it?" Neither can answer the question "Why should I do it?" except on the basis of postulates arising from some source external to science. A social organization can be

judged satisfactory or unsatisfactory only in terms of such external postulates.

In these United States the traditional postulates are formulated in the Declaration of Independence to which we all do lip service. We sum them up under the cliché "democracy." I personally, have been so conditioned by training that the essence of these postulates seem to me to represent the only basis upon which a permanently satisfactory social organization can be based. However, I seek in vain for any sound scientific basis for this belief. "Lord, I believe, help thou mine unbelief."

Granted this firmly entrenched belief, I look around over this, my country, and am grieved to see how far we fall short of the ideals set up in these postulates and I ask myself wherein I as a scientist, or scientists as a group, can assist in their more perfect realization. How can science or scientists aid in implementing democracy? Can science or scientists persuade the people of this country, and more particularly the leaders of public opinion really to want the fullest possible development of democracy in it and not merely say they do?

Perhaps psychology can answer that question.

Granted that we really want democracy, how can we best obtain the adequate free and effective expression of the popular will, which is one of the fundamental postulates underlying democracy? Here, I think, science can help. Since the days of the French revolution able mathematicians have devoted much thought to the theory of voting. Such names as Laplace, Lagrange, Condorcet, and Nathanson may be mentioned.

Out of it has come a body of theory well verified in practice which is practically ignored in our usual system of election. Only in a few of our cities has any application of these theories been made, but where applied (Cincinnati for example), they have in almost all cases resulted in more satisfactory social conditions.

Ten Lectures and Discussions on

SCIENCE: Its history, Philosophy,

and Relation to Democracy

held at the Department of
Agricultural Graduate School
under the chairmanship of

M. L. Wilson
Under Secretary of Agriculture

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Edited by W. Edwards Deming
Assistant Director of the Graduate School

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Lecture X

THE PLACE OF SCIENCE IN DEMOCRACY

by

H. Levy
Professor of Mathematics
The Imperial College of Science and Technology
London

The Graduate School
The Department of Agriculture
Washington
1939

THE PLACE OF SCIENCE IN DEMOCRACY

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In the auditorium

I

It is rather a venturesome thing for a scientific man to step beyond the restricted boundaries of scientific investigation to deal with a problem that in the past at any rate, has been essentially within the province of the sociologist and the philosopher; but in this world of science, for good or ill, we have come to recognize that the walls which have artificially separated off the internal field of science from its wider geography no longer stand as clearly built, as solid as they did. What happens internally has repercussions externally which have sometimes been rather startling, and vice versa. Unless we can study this subject of science internally and externally and then bring the two together, we shall have a completely false view of what it means to us. I take it that the purpose of this course is precisely to study that particular problem.

Now, it is not as if the subject has been broached for the first time. Many philosophers have already become weary and wary of such problems. There is a school of philosophy of which many of you must know, the logical positivists, who consider that the essential problem is to discover how to ask a question. They are not concerned with the actual question so much as the way in which the question should be asked. While I am not myself concerned to say that that is not an important thing--indeed I am with them in the sense that the kind of answer you get depends on the kind of question you ask--yet I do still feel that there is not anything very surprising in the fact that there should develop, at this particular time in the history of the world, and particularly in the history of Europe, such a school of philosophers. Today in Europe it is rather dangerous to ask questions; it is much safer to discuss how a question should be asked. And so if the implication is valid, the situation in Europe today seems to suggest that the focus of interest of philosophers at any rate is very closely conditioned by the general state of affairs in which they live.

That story is also to some extent the story of physical and of biological science. We have come to recognize that the focus of interest of scientific men has changed throughout the ages, and in various environments. It has changed in space and in time. Thus the study of scientific problems, the analysis of the actual physical processes that take place, raises in itself also a problem relating to the study of science. When scientific men examine this problem and that, in this context and that, when they shift the focus, we should expect to see something of the same type of regularity in that movement as we find in any other natural process.

II

The activities of scientific men and the activities of all those people in the community who apply science to production, to technological, social, and human ends, all are themselves objective processes in nature, part of a changing society, part of nature; and who will say there is any aspect of nature which is not amenable to scientific study?*

But we have to be clear about the nature of the analysis. The method that we apply to the study of the physical sciences is not, as it stands, entirely applicable to the study of the biological sciences. If you apply, for example, the objective measurable methods which you learned in chemistry and physics to biology, then naturally your method will act as a sort of sieve through which will pass those problems in the biological world that are not amenable to that kind of treatment, and you will sort out of the world of biology just those problems that can be classified as physical or chemical, even though they have biological forms.

If you are to turn your attention to problems of a biological nature other than physical or chemical, then you will have to extend your methodology in new directions. New methods of approach have to be taken and new concepts pieced together in a new logical and scientific way. The mind of the scientific man has to adapt itself to the changing material and the new forms of behavior that have to be examined. So if we imagine that the present methods of physical, chemical, and biological science suffice in themselves for a study of the social problems of science, indeed that particular social problem that consists of the study of the shifting focus of science, then we may make a very great mistake. We have to see the scientific method itself expanding and developing with the expansion and development of the material to which scientists turn their attention.

Having said that, we begin to see that the methodology and the scope of science is a historically changing phenomenon, and that both are likely to be very closely conditioned by the social environments in which they have developed in the past. We have the impression, you see, approaching science from the inside, that we are merely attempting to extend the boundaries of knowledge; we are attempting to accumulate as much information as possible about the objective world around us, and that we approach this in a completely unbiased, completely objective way. Judgments and valuations, likes and dislikes, bias and prejudice, play no part. That, at any rate, is the kind of theory that is felt to express the methods which scientific men apply. So far as scientific men are conscious of the existence of bias and prejudice when they occur, I would be the first person to say that they dismiss them from the problem entirely, but there is such a thing as an unconscious world of thought and feeling. That is a matter for investigation by psychologists and sociologists. There are many unconscious pre-judgments which we introduce; although we are unaware of them, they nevertheless show themselves in our actions.

* Cf. Professor Conklin's remark at the top of p. 73.

III

How do these things enter into the field of science? Supposing we were to construct what I will call a spectrum of scientific knowledge. Let us say we have an extended band and we place the amount of information we have in the different scientific fields along it. We will begin, if you like, with the mathematical sciences at one end, passing through the physical sciences, through the chemical and the biological sciences, to the psychological and the sociological sciences. We can imagine that this spectrum shows the intensity of knowledge--if you like, the intensity of energy--that is being devoted to study in each of these fields. At the present moment we could map out such a spectrum if we had the information. It is an extraordinary thing that scientific men have little information about science. Here is a picture of the activity of science which you would imagine we would be keeping continually before us, and yet there does not really exist full information that would enable us to map out that spectrum. Who can tell what we are doing to the community; who can tell what we are doing to democracy with our science, unless we have a balanced picture of the science that is? But that is not all.

Each of these particular fields represents the energy of human beings. At one point you have so many thousands of people in different countries devoting their time and attention and public finance to the maintenance of study in the corresponding fields. Which particular fields are being encouraged to the detriment of others? Which particular fields should be encouraged; and I underline the word "should". It involves a judgment of some kind.

As soon as we use the word "should" in this connection, we are suggesting that science functions for some purpose; that if you are going to make science function for a purpose, then you will begin to allocate your funds in some consciously purposeful way across that spectrum; you will allocate the energy of scientific men according to a particular kind of distribution. If we were conscious of the purpose of science in democracy, the purpose of science in the community, if we knew why we were pursuing science and what our conscious objective is, if, in fact, we could plan it socially, then we should be able to say, "On such and such a principle it is quite clear that the distribution of energy should be so and so; our funds in the future will, roughly speaking, follow such and such a form of distribution." I am not suggesting, of course, that on a matter like that you can have the kind of detailed and accurate knowledge or the planned arrangement that you can have in the self-contained, controlled experiment in the field of physical science. What I am really suggesting is that we should begin the scientific study of this question sensibly. If we are to talk about the function of science in democracy, we must have the data on science, and the sooner we can accumulate that data and determine where that energy is distributed across the spectrum, the sooner we shall be in a position to talk about what science can do for democracy and what democracy can do with science.

We know that in the ordinary spectrum there are ultra-violet and infra-red sections; there are rays that can not be sensed in the ordinary way. In the same way there are bands of scientific knowledge that have not yet been touched, there are sections of the spectrum that have not been opened. So far as they are untouched they lie in the future, but they represent certain untapped resources of energy that man can still set about to apply to the future development of democracy; but unless we set about it in a systematic way, we shall be unable to apply them to the fullest and most scientific extent.

IV

This spectrum has changed throughout the ages. In what way has it changed? What is it that settles the nature of that spectrum? I am not going back over the history of this matter. It must have been dealt with to some extent in previous lectures, but you can imagine the kind of spectrum that could have been drawn up, shall we say, for the early agricultural stage of human society, embodying a knowledge of tools, the implements for the tilling of the soil, the biological understanding which an agricultural community would accumulate in attempting to pursue its activities in such a way as to be certain of the outcome of its work. For the pursuit of science is the pursuit of certainty. It is an attempt to make what is fickle and unsure into a certainty. What lies in the future is uncertain: what lies in the past is certain, for it has already happened. We apply probability to the future: we apply determinism to the past. And the whole of science is an effort to apply to the future what has already been applied to the past, to make the future as determinative as possible; to be able to plan in a conscious way, satisfied what the outcome will be.*

Thus science and the scientific spectrum of the agricultural period would embody the kind of knowledge, experience, and technological application that the people of that period would require. So also with the feudal period, so also a distinctive distribution during the mercantile period, during the early industrial period, and during the modern period of finance and industry. Each particular state of historical development of man would correspond to a particular color distribution of scientific knowledge built always on the knowledge of the previous stage. The history of man is the history of human beings accumulating experience about the world and changing it in the process, and one aspect of that experience is scientific knowledge and practice.

When we see it that way, two things stand out. We see first the fallacy of imagining that science is a thing in itself, that science stands outside human endeavor, that science has not been created by man in his struggle with nature. Secondly we see how

* Compare with Professor Morris' statement that the test of the reliability of knowledge is accuracy in prediction; p. 160.

science has passed from one level of achievement to another, each level building itself on the previous one. In the early stages, man struggles with physical nature. He passes on to use that knowledge to struggle later with biological nature, the rearing and domestication of animals, the raising of crops, and finally with the biological problems of man himself. Problems of method are aroused. He begins to be faced with urgent sociological problems and he concentrates on the social evolution of man. Finally he is driven to a study of the interaction between these restrictive fields which themselves emanate from the struggle of man,man himself as a member of the social group. The very fact that today we can meet here to discuss science and democracy is an indication that we are now striving to find a footing at a new stage of human development.

V

In 1660 the Royal Society was established in London to study physical nature and its processes. That period was a very significant one in the history of the west. It marked the beginning of the industrial revolution, the very early days of the coming of machinery to the west. Not consciously for that purpose perhaps, but nevertheless apparently so when historically seen, the study of physical problems was pursued with great vigour, and we see historically the outcome of the studies applied in due course to the development of machinery on a phenomenally increasing and enhanced scale. The society of men to which that has been applied itself undergoes transformation. A Europe that was part feudal and part mercantile became transformed into a Europe of industrial tenements, slums, workshops, factories, wealth and poverty, with all the outlook, judgments, and values that men, women, and children acquire when they live in congested towns.

Those thoughts are associated with 1660; and today we are apparently on the eve of new changes. We meet here, as meetings are being held like this almost in every civilized country in the world, to discuss what? To discuss the relations of science to society. The focus of men's minds is being turned to the social aspects, the social repercussions of that science; and instead of now concentrating entirely on the old spectrum of scientific knowledge and its internal content, we are now beginning to ask: just exactly what has all this done to the society in which we live? We are breaking into the ultra-violet and the infra-red. Questions such as these could be asked in sensible form only when those other aspects of science have worked out in such a way as to transform the outlook of man. It has been an experiment on mankind which has now reached such a stage of development that it has produced men who can look back on the experiment and begin to examine it. We have reached a new level of scientific understanding because we are able to pose a deeper problem about nature. When we examine this new problem, science in a democracy, in civilization, and in social life, the danger that we are faced with is of the kind that philosophers put their finger on when they say, "You have to be very careful how you use words." When we talk about science and democracy some of us tend to think of democracy as being the summation of the highest form

of social organization that could have been produced throughout the whole of man's history, and so we think of democracy as being the last word. Yet if we go back to the 1660s when the Royal Society was founded, before the industrial revolution in Eurpoe, there was no democracy in the modern sense.

Democracy is something that comes into being when a country becomes industrialized, when science is applied to develop industry, to start factories, to bring people into towns to work in these factories. We cannot have a population working in highly technical processes, complicated machines, read blue prints, carry through the necessary calculations to organize and devise highly skilled mechanical jobs in large scale production, without educating the population. If we go back over the history of England, we find that the principal educational developments that took place in that country occurred during the period of industrialization, and that it was during this period also that the people of England got their democratic rights, their rights to vote, their rights of free speech and free industrial association. The great drive for technical education in England arose during the early days in the 19th century. All these things followed in the train of industrialization.

It was no accident, but a necessary corollary. We have to think of democracy as being the outcome of a technical age. The difficulty is, that having just found ourselves in a democracy, we tend to make a kind of abstraction of it from its historical and social context, and talk about it as something absolute. If we will think of democracy as the outcome of a technical age, then we can see the changing forms in which human beings have aggregated themselves together in society. The kind of institutions they have to set up to carry through their work, and to express their human activities, legal, religious, scientific, political, distinctive for this period, have been built up since the technoligical age came into being, and these we call democratic institutions. Science develops as part of this historically changing process.

VI

The question we are asking is this: exactly what is the function of science within this democracy? And we are thinking of the one growing within the other, each influencing the other.

A difficulty arises just as soon as we raise a question like that. If you take an ordinary physical problem, like the kicking of a football, (or should I say the striking of a baseball?), then we think of force being applied to the ball. The force is external. After the blow, the ball goes off with some measurable speed. The whole analysis of physical problems in the past and into comparatively recent time has been the cause-effect analysis; single cause and single effect. That kind of analysis, that outlook, has impregnated our modes of orderly description. Even if we discuss human beings we say, "So and so is responsible for that," that is to say, we blame him for this, we make him the sole cause. We isolate

him causally from the whole universe and do not regard him simply as part of the universe.

The isolation of cause and effect, in the restricted sense in which we apply it in physical science, cannot possibly be the whole story when we investigate social problems. That is what I meant earlier* when I said that the methodology of physical science must not be applied in a mechanical way to the solution of these other problems. I might maintain on the basis of what I have already said that since science is the outcome of a developing society, society is the cause and science is the effect. It is equally clear, however, that science applied to society transforms it, changes it drastically. Think of the changes that came over the British people in the 19th century during the industrial revolution and you will realize how much science and technology change society. Thus science now becomes the cause and the changes in society are the effects. So here is society, on the one hand the cause, and science the effect; and on the other hand here is science the cause and the changes in society the effect. Which is cause and which is effect? It is like a dog chasing a rabbit. Is the rabbit running away from the dog, or is the dog running after the rabbit? Quite clearly, cause and effect cannot be disentangled in this mechanical way. The two are united for certain restricted purposes. Look at it from one end and see science and society as cause and effect, or from the other end and see society and science as cause and effect; when you look at it from either end you are taking one aspect or one slice of a composite situation. It is important to do that of course.

It is important to examine which particular scientific applications to social life are likely to have which social effects, and it is also important to know what social features are likely to develop or to frustrate science. For example, today in Europe we have a very tense international situation. That illustrates in a rather remarkable way this mutual conditioning of science and society. It is always a good principle in experimental science that if you want to investigate a certain characteristic, set up your experiment in such a way as to accentuate it so that you can measure it accurately, and see it in all its details. If you want to see the effect that society has on science, examine that science during a period of social stress. Look at scientific activities today in Europe, and probably I should imagine also in the United States: certain parts of the spectrum of activity I referred to have suddenly acquired new energy. Scientific energy is being expended on the investigation of problems of defence and destruction of life and property. We are not discussing just now whether this is a good thing or a bad thing; we are not discussing values. It is the objective analysis of science in its social relations with which we are concerned. But it is quite clear that today in Europe the stress of this international situation--a state of social stress--is forcing scientific men to concentrate their energies and their

* At the commencement of section II.

attention on particular classes of scientific problems.

Here then is the effect of society on science, and if it is desirable to study that problem, as I personally think it is, then now is the time to study it, when the situation is such that the effect on science is exaggerated in comparison with what it would be during normal times. Now is the time to draw conclusions regarding the nature of the linkage between science and society.

All this concerns the effect of society on science. But there is another effect that, it seems to me, is showing itself today. What is it that terrifies every government in Europe? War. They see the possible destruction of highly industrialized communities, congested towns like London, Berlin, Paris, elaborate machinery, expensive buildings, and complex commercial organizations to which an enormous amount of social energy has been devoted. These represent the creations of scientific knowledge applied to technological ends. Is modern society stable enough to withstand such an onslaught? Science in its application to war has created weapons of such destructive capacity, has made such devastating inroads into the problem of destroying life and property, that every government in Europe today is terrified of the consequences. They may be using that terror to threaten each other, as a political maneuver, but that is another matter.

Here, then is the effect of science on society. Here you have a sort of European paralysis in the political and social field. Thus here are two effects of science on social affairs, first, the application of science to general technological ends creating industrialized towns and cities; and second, the application of science to weapons of destruction, so that every government stands in fear of the outcome. I am suggesting that today then, since the social situation is intense, you must expect these characteristics of the interrelationship between science and society to show themselves in an extravagant form; but I am also suggesting that extravagant as that form is today, the linkage has always existed in the past.

It has not always been direct. There are some people, for example, who see only one aspect of this question. My friend, Professor Hogben writes on this subject, and he stresses the view that science is produced primarily to satisfy social needs, and that for whatever social needs there are at any particular period in history, then finally, sooner or later, the necessary kinds of science will be produced to satisfy those needs.

It is a very simple thesis, but to me that is only half the story. Science satisfying social needs is just one side of the picture. Science also creates social needs. Science brings new needs into existence. If you can create wireless; people begin to want receiving sets. The balance of necessaries and luxuries is altered. There is a shift in

social needs in the wake of the application of science. Moreover, the relationship is not simple and direct. Precisely what are the social needs that are satisfied by a study of the theory of relativity? Which particular technological application can one offer to show the importance of the theory of relativity? If it were true that science is important, only so far as it has social application in that rather crude sense, then it wouldn't be possible for people at one and the same time to say, "Why, the most important development today in society is either the bombing machine or the radio, and the greatest scientist today is Einstein." The reference here is to two fields; one, rather abstract, which deals mainly with the logical structure of scientific description; the other, dealing essentially with concrete sociological application.*

VII

In the early stages of man's struggle with nature, the relation between science and society is obvious. Man had to make a tool to overcome an immediate practical difficulty, and he applied whatever knowledge he had to that end. A time arose, however, when his social life became more complex. He no longer immediately consumed what he produced, but put off the consumption for a rainy day. Thrift and self-denial emerge as social virtues in order that the community may survive the dangers of the future. In the same way he studies not merely the immediate application of science to a constructive end but the properties of this or that which may be useful in the future. Thus there emerges the profession of the scientist, and the logically interwoven fabric of theory and experiment. It is in that that relativity finds its place. Accordingly a second order relationship also manifests itself between science and society.

That theme could be developed much further. The history of mathematics, and modern mathematics in particular, bears out the fact that the development of the more theoretical sciences comes as a second stage. It has also an immediate social significance. It is important intellectually, emotionally, and therefore socially, to men. Sums of money are devoted to setting up chairs in very abstract branches of science or of mathematics. It is felt to be good educationally. It is demanded by a certain traditional need, or an emotional, intellectual, or moral need. Call it what you will but it becomes also a social need which must be satisfied. When we talk, therefore, about the relation between society and science, we shall make a gross mistake if we take an entirely mechanical view of the connection.

All this has arisen because I have been trying to see how far one would be entitled to take the old methodology of science, cause and effect, and apply it to a social problem. We have seen that this method is in fact

inapplicable if we try to understand the changes in society and in science in their proper perspective, in their entirety, and in their interconnection.

VIII

What exactly, then, have we to do? What kind of methodology should we use? In what direction should we change the form of analysis of physical science? Well, I think that we have to apply consciously what we have unconsciously applied in the past. Suppose we take a history book and turn over the pages. The first few are devoted to primitive society, and then a large number of pages to the feudal period, a considerable space to the mercantile era, and finally we are led to the industrial stage. Thus we see a procession of human beings passing across the pages of history, transforming themselves and their environment as they go from one stage of society to another, each particular stage showing its own spectrum of scientific knowledge, its own particular grouping of technical application, its own system of moral judgment, its own complex of religious beliefs. These represent the social development at any one time. It is a process in nature, and we can examine it to see why it was that each particular stage was transformed into the next one. It was not done by miracles. It was the activity of human beings that transformed the one into the other. Here is a changing pattern in human activity, and we can analyze it in a restricted way into cause and effect.

The needs of the feudal system necessarily called for the craftsmen and merchants; craftsmen to make harness, the fighting weapons and the coats of mail; merchants to bring the spices, scents, and oils from the East and exchange them for the commodities created by the craftsmen. The feudal system forced craftsmen and merchants into existence out of sheer necessity. Then these people developed an independence and a social power of their own.

The next stage emerges as the merchants and craftsmen rid themselves of the feudal lords when the latter become a drag on their development. They themselves become the dominant group in the community; they develop new knowledge from trades, crafts, and navigation. A new band of scientific knowledge is added to the scientific spectrum as a new society is being built up. We can see an internal cause-effect relationship, but in addition to that we can perceive the growing social pattern, and the changing form of combined cause and effect.

It is the interrelation between science and society, between science and democracy, that we must also follow as it creates a growing pattern. We ask ourselves what kind of science was produced in the early days of democracy, and what was its reaction on democracy? At what stage was that relation transformed into the next stage of democracy, with the next stage of science, and so on? What are going to be the successive kinds of socio-scientific patterns? Each of these can be broken up internally into cause-effect relations of the kind we are accustomed to deal with in physical sci-

ence, but here we are dealing with a dynamic pattern that subsumes that analysis. I cannot go into this tonight, but it does seem to me that along these directions lies the analysis of this problem, and so far as we feel today that such questions are important we become as it were, the agent which brings the new kind of analysis into existence.

IX

We are entering a new phase in scientific methodology. We are working out a new part of that pattern. Exactly the same process is shaping itself now as I have tried to indicate can be seen in social changes. In 1660, and for nearly three centuries thereafter, it was merely a case of applying scientific knowledge to the making of machines, to the directing of physical energy; but now we have reached the next stage where the relation between science and democracy, science and the people, has become one of the people transforming the people themselves, planning the future of democracy. It is a tremendous thing that we are undertaking. We are actually beginning to plan our own future, stretching out to make a new people. We now ask what kind of democracy can be planned in the future, and to achieve that purpose what kind of a stress has to be laid on what kind of scientific work? Which field shall be encouraged and which shall not?

It is a very curious thing that this particular problem--it is a scientific problem of a new type--is one in which nobody is paid to study. You can ask a man to become a chemist, a physicist, or a mathematician with a fair certainty that someone will find his knowledge valuable enough to pay for, but if you say, "Go now and study the relations between science and society," he can legitimately reply, "Who wants to know that? What is the good of it? I see the importance of it, for society, but who wants to know it? Who is going to apply it?" We have to be very careful, for science cannot live by theory alone. We must understand exactly what we are doing in this matter. Are we studying the relations of science and society, and of science and democracy, as a part of an intellectual amusement, something to be talked about in an evening, or is there some objective method of applying it consciously to democracy?

The present stage is one in which the physical and biological sciences are those that it pays men to study. There are posts in these fields. Their work finds fruitful, remunerative application. People have to live. Young men today who investigate scientific problems like to see their scientific papers published in scientific journals. It is very important when a man applies for a post to be able to say, "I have published so many papers." Suppose he said, however, "I have published a paper on the relation of science to society, or on the effect of certain physical discoveries on unemployment." Precisely how much credit would a mathematical physicist or chemist or an engineer be given for a paper of that nature? Very little, indeed. You may say that is not his subject. Well, whose subject is it? It is remarkable and encouraging that in the United States

there appear to be quite a number of universities and government departments that are sufficiently keenly interested in this topic, to want to develop a professional avenue for it and even to see some of its findings applied.

In England it is done as a spare-time hobby by some people who are vividly conscious of its urgency. We have to try to find time for it from among a multitude of other tasks. In practice its importance is unrecognized. Nor have we yet the possibility of implementing our findings in legislature or governmental activity. That kind of knowledge, necessary as it is, finds no immediate wholesale encouragement. It is true that the British Association of Science this year has set up a new division* especially for its study, but one of the difficulties we are encountering lies just in this fact. Those people who conduct such investigations ought to be given the same kind of status when they publish their results as is given to scientific men in other fields. That tradition has yet to be built up. At the present moment it can be encouraged only by the actions of public spirited men of high social consciousness.

I end therefore on the note that if this subject is to have the effect socially that the study of physical and biological sciences has had in the past, then somehow or other those people who are in the position to shape public opinion and to shape scientific opinion will have to see to it that the younger generation, keen as they are on the study of these questions, shall have the opportunity of receiving for these investigations the same kind of credit as they receive in other branches of science. In that way the historical epoch of 1660 will be repeated three hundred years later at a higher level of social understanding.

* This new division is often referred to as "P. E. P.", for Political and Economic Planning. Dr. Moulton referred to it on p. 6.

SEMINAR

following the lecture by
Dr. H. Levy

Office of the Under Secretary

Syllabus

1. Research in industry is conditioned by the fact that the members of the staff are trained in academic institutions; p. 254.

2. A planned scientific research program is possible in a democracy, and moreover, the democratic method brings forth a cumminity that understands the plan, sympathizes with it, and helps it forward. P. 255.

3. The fundamental question is how we can bring about a conscious plan for the energy distribution in the scientific spectrum. The leaders must have extremely broad training. P. 256.

4. Planning is not a static affair, but a constantly changing one. P. 255.

5. Democracy may take various forms in different countries at first, but it must converge ultimately to the same thing; pp. 259, 265. Scientific men should do their part in insisting that technical posts in the government service be non-political; p. 259.

6. Scientific men must share their knowledge with the community by applying the scientific method to problems of community interest. P. 261.

7. Society is a changing pattern; any experience that a man has changes the man. In this way society differs from a physical experiment or a machine. These are able to repeat the same results over and over again; yet there are definite laws of change relating to human behavior. P. 262.

8. The trend of science is not purposeless. At each stage in history the purpose has been the creation of the next stage. P. 264.

9. Democracy has grown out of the industrial system; p. 265.

Proceedings of the seminar

DR. BLAISDELL: Dr. Levy, this group gathers informally once a week with the previous day's lecturer to discuss with him some of the implications of the lecture. We try to get an informal discussion started to clarify and bring out in more detail the previous day's lecture.

Yesterday, if I understood you correctly, you presented the concept of a dynamic and a changing methodology in science, and moreover you talked about a state of dependence between science and society rather than one of mechanical cause and effect. I got those points, at least, out of what you said. They were extremely interesting to me and I just throw them out here as possibly a starting point for our discussion today.

DR. WINTERS: Dr. Levy, you discussed the effect of different sciences and the interrelationship of the different sciences on society, then the need for a study of what these sciences as a group had done to scoiety. In the meantime you discussed the possible need for the redistribution of energy in the spectrum of science for present needs. Would you mind expressimg some opinion with reference to what you think or how you think our sciences should be redirected for the benefit of society?

DR. LEVY: There are two points one has to bear in mind in that connection. First, one has to think of the new fields of inquiry that have to be stimulated and that have been allowed to lapse or have not yet been investigated properly in the past. That is a field associated primarily with just the very question we are talking about, the interrelation of science and society; it would throw light on the whole problem of the administration of science in a community. Second, science in its relation to society has been bringing out the necessity for a more thorough-going scientific administration of its discovery and application throughout the community rather than its being used in the hands of this little group and that little group in the way in which that little group thinks it is best either for itself or for the community. It is difficult to make a complete illustration because we haven't got to the stage yet, but I can give you an indication of what I mean, and naturally I must draw on British experience because it is the only experience I have. Let us take the British Broadcasting Corporation, for example, which is a body that runs the whole wireless system of the country. It is not a private company; it is a public company, if I may use the expression. It is simply a corporate body which has a statutory basis, I would say. It has been set up by Parliament. It employs people to give amusement and lectures and educational talks, and so on. It has a staff investigating the technique of broadcast production. It has a scientific staff. It has an administrative staff. It is studying the problems of administration of such a department, and so on. It is studying its internal scientific problems. Now, such a department is actually producing. It is producing and purveying the goods in the form of amusement, talks or intellectual stimulus, and so on. What is its economics? Well, it receives its income from part of the money which is

paid by the people who receive it, that is, receive the goods. That is to say, if I have a wireless set, I pay a license of about $3 per year. That license goes to the government, part of it goes to the British Broadcasting Corporation, and the sum total of that is their income, and then they expend that in their own way to carry through its public service, and it is all expended either in the form of wages and salaries to the people who are running the concern or in the form of further scientific development to enable it to be raised to a higher technical level and give better service to the community. That is what we call a public body. It is a public service. There is no particular individual to whom it belongs. It belongs to us all. And if it is to be criticized and examined from the public standpoint, it is criticized and examined on the public forum in the House of Commons or on public platforms. So far as we have freedom of speech and so on, we can criticize and discuss it, but it has to be criticized and discussed in an enlightened way because they have the power of reply in the sense that they have the broadcast. That is the kind of thing that democracies are producing, and it gives a tremendous opening to the application of science to social service. In the past we have thought of social service as being an individual thing, an individual or a small group helping people who are in distress, but we can transform the concept of social service into the community, doing the services for the community which are not in the first place so easily or so effectively done by small private groups. The expansion of that concept is certain to transform the whole relationship of science to society, during the process transforming them both. I don't know whether that is an answer.

DR. BLAISDELL: Do I gather, then, that you would argue for public subsidization of scientific research ultimately to replace privately endowed research?

DR. LEVY: Well, I wouldn't go so far as that. I think that might come afterwards. To me it is immaterial whether the funds come from this particular source or from a general source. The community has produced it and is producing it, and the fact that it comes through private hands rather than through public hands is merely one of your methods of warping the distribution of energy in the spectrum. You are allowing this particular group which possesses the finance to determine which scientific endeavor shall be encouraged. If you think that is the most scientific way to do it, well and good. I should have thought it was better to plan it.

DR. STEVENS: Dr. Levy, there is a question I should like to ask in regard to the motivating forces in the conduct of scientific work, comparing the profit motive with the public service motive. Compare our research or our motives with those of the research conducted by the Bell Telephone, du-Pont, General Motors, General Electric, and any number of the large corporations. I think it is fair to presume that the main drive for their research is the profit motive eventually, although they do a great deal of fundamental research which has no immediate and visible application. I should like your views as to the comparative benefits to society of conducting research by the profit motive on the one hand and the public service on the other.

DR. LEVY: Well, it is a very difficult thing to estimate. One can only give a judgment, not a scientific answer. In the first place I believe that an examination of the direction in which science is being financed and is going to be financed will indicate that the profit motive as a driving force for the prosecution of research is likely to be gradually substituted by the motive of public administration and public service. I am making that as a statement for myself, but I think that is what is indicated by the trend of events. That is not belied by the fact that there are growing large organizations in industry which are devoting more and more money to research. That must be taken as part of the general trend to devote more and more money to research altogether. The next point is whether you get better results from the private endowment of research. Now, that is not a simple question in the way in which you have asked it, because private endowments of research operate in an atmosphere in which science has attained a certain academic standard with regard to the pursuit of knowledge, and it would be impossible at the present moment to run any private industry with a research laboratory in which you did not bring in people who had the tradition of academic research besides having to apply that tradition of their work to a limit.

Take a well known electrical concern in England, for example, which makes lamps and valves and wireless sets, etc. Well, now, the atmosphere of research in this concern is almost the atmosphere of a government research laboratory or a university research laboratory--almost. There is a certain stress in the application of the industry, which is natural. You expect that. But the company doesn't take people from birth and train them up to the laboratory; they draw them from universities and other academic institutions, and from the National Physical Laboratory, and so they have university electricians and university professors, who have been attracted to come there. These privately endowed research laboratories working for industry necessarily have a personnel with an academic background and with an academic tradition.

DR. SEEGER: Dr. Levy, I found your talk last night so stimulating that it raises a couple of questions in my mind. Whenever we start out planning, we must have an objective in mind. Then it seems to me that you are getting something that is likely to be static, and that as a matter of fact the group that has a plan in mind will tend to keep that plan going, and the question that arises in my mind is who is to do the planning, and particularly is that possible in a democracy? Who is to choose the objectives of the plan, and who is to determine when the plan is to be changed in a democracy?

DR. LEVY: Well, the point you are raising is of fine importance, but I think the answer is to be found not by asking me but by trying it. This isn't a theoretical point we are discussing. We are discussing the theory of a practical point. Even the planning of an experiment is a tentative thing, and as soon as you put the apparatus up and you discover you have omitted to take account of such and such a fact which turns out to be important, you reconstruct your plan to that extent. I am assuming this is done in a scientific way, and therefore I am assuming that the people who plan, and the admin-

istrators concerned with it, have a scientific understanding of what they are doing and transform their plan as they go along. They must understand the relationahip of themselves to the plan and of the plan to the community. The objective that they have in mind undergoes a change of the patterns we were talking about last night, cause and effect, and the whole thing being perpetually transformed. If we can bear that in mind as a necessary process of change in a plan, then we get away from a static concept. We under-cut it by saying, "Let us think in advance what are likely to be the successive plans through which we have to go." As soon as we think of that, we begin to change it. Who is to make a plan? In a democracy there is a government that is determined presumably by the will of the people who have to be educated to understand what advantages can accrue to the people by scientific planning within a limited field, but the right to do that must come from the people through the government to the particular planning commission, but it is limited in the rights it acquires.

DR. SEEGER: I can see how this could be very effective in a totalitarian state, but--I am just thinking out loud--it seems to me that a democracy is not a very efficient way of achieving scientific planning.

DR. LEVY: Well, I don't know. What you are saying is that in a totalitarian state in which your population is regimented, you could carry through your plan.

DR. SEEGER: Yes.

DR. LEVY: I say that is a static way of looking at it because if your people are regimented they presently will not like your plan and that plan will undergo a transformation as people begin to resist its application. A totalitarian state is a temporary phase where people have been made into machines. That is the kind of planning which will have to be transformed. You have to think of a sequence of plans in a totalitarian state. We are talking about a sequence of plans in a democracy. You may think one in the first stages may be more efficient than the other. I am suggesting that the democratic method is likely to give you a community afterwards which really understands the objective of the plan, sympathizes with it, and helps it forward. We needn't discuss the relative merits of a totalitarian planning and a democratic planning. We have a democracy. If you are suggesting we shouldn't plan in a democracy, you are suggesting that we should carry on our work with less intelligence and understanding than we might otherwise do.

MR. TRULLINGER: Following that line of thought, Dr. Levy, I believe it would be interesting to get your opinion on how it is possible in a democracy to keep the right balance of emphasis on the importance of scientific work or scientific research. You built yesterday a most dramatic picture of the chromatic scale, the color spectrum, a picture of emphasis on the various features of scientific endeavor--scientific research and scientific thinking--which to us means very largely a picture of the amount of financial support. That financial support represents the distribution of the interests of so-

ciety, and in a democracy, as we see it, that interest of society is built up on very definite needs. During the World War, early in the World War, the scientific minds of the Central Powers were in considerable disrepute because of their unusual attainment in the application of science to military procedure. They were looked upon as wearing horns and things of that kind. A little later, however, the scientists of the Allies met science with superior science, and the result is history. In other words, as we see it, the emphasis on science and the status that science carries in the public mind is likely to go up or down, and is almost sure to be expressed in terms of financial support. I should like to ask whether it is worth while to have a balance of support so that these different phases of the spectrum will balance each other, and how in a democracy that can best be brought about.

DR. LEVY: Well, that is, of course, the whole problem. In the past we have had a spectrum, but it has not been planned. It has just come about by higgledy-piggledy endeavors of this little group and that little group. With the government departments there has been a plan in a sense, but an unconscious plan. How can we make a conscious plan? We can make a conscious plan to the extent to which we understand what we are trying to do, and therefore the plan must go on approximating greater and greater clarity as we understand more and more. Who can understand and who can convey the understanding? In their own limited field, scientific men understand. It wouldn't be an impossible thing to take all the people here and get comparative unanimity about what the distribution should be, provided, of course, their own personal interests were not involved. Now, however, if you group the agricultural, physical, chemical, and biological sciences with the social sciences, it would be different. The people would say,"Well, I don't really know exactly what the balance between these various things is." And yet there are people who are obliged to make such judgments. How, therefore, can we enable the people who have to make a judgment to make a surer judgment? The way to do that surely is for those people who have received training in one field to receive training also in another field on a wider setting so they can unify the two.

Let me take again for that purpose an industrial concern where they have a research laboratory, and a works or development laboratory. The scientific work done in the research laboratory is applied in the development laboratory before it goes to the factory, and then finally you have the factory. You have the three sections--the scientific laboratory, the development laboratory, and the factory. Each of these and together must be administered, and they have to be administered by people who understand the technique in all three sections but particularly of one section, so they are specialists in one and have a general understanding of the other. They must understand administrative problems. It is common experience that you cannot bring an administrator with no scientific knowledge into a problem of that kind, and it is common knowledge that you cannot take a scientific man deeply engrossed in science and punch him into an administrator when he doesn't want to be one. At the same time it is also common knowledge that the task of the man who has to coordinate all of the lot is one which de-

mands both kinds of experience, and it becomes merely a question of educational practice how you have to train your men to have the knowledge of the one and the experience of the other.

A scheme I outlined some years ago was to take young men at the early stage when they came in as research assistants; those of them who were interested even to a slight extent in administration were given a small administrative job to do along with their scientific work, and then if they seemed to be broadening their minds at the same time, not remaining the narrow and deep specialist but having the broader type of understanding, they were given more administrative work, but they always retained a certain proportion of scientific work to do, and so out of it we got administrators who are still doing scientific work. If you take a first-rate scientific man and make him entirely into an administrator, my view is that you have made a tremendous mistake. A student going into the university, taking a degree in a scientific subject, should come out with not only a philosophical understanding of his subject but a social understanding of his subject; and these philosophical and social notions must be part of his general educational training and his scientific training, otherwise you won't get the kind of men that democracy needs in order to expand and carry out these plans. When you have the right kind of men, you have people in position to interpret the technical side of science and to carry it through to your political chiefs or whoever they are, to see that these things are clearly explained to them and implemented in the right way. However your democracy is organized, these people also become publicists to the outside world, to explain what science and scientific administration can do for the common weal. It seems to me that you must move in that direction, that is, building up organization after organization, but the educational drive must come from the people who understand, and for the moment a limited group of scientific men have that vision.

DR. KELLOGG: It seems to me that theoretically that is excellent, but I wonder how we are going to prevent ideas coming from the top? You are talking about building up, becoming more and more sympathetic as it comes up. How are you going to prevent ideas of what should be done being determined at the top, and the people who have excellent scientific jargon then more or less twisting and justifying their aims through scientific jargon rather than clarifying them through the application of scientific analysis, as you are suggesting. Isn't that a danger?

DR. LEVY: Yes, there is a danger, of course, but it is only a temporary danger. It is the growing pains of our democracy. I remember the early days of the National Physical Laboratory in England, when the whole administrative staff consisted of people who had never seen the inside of a scientific text book. Then later the scientific staff grew up mainly under the impetus of the war, and the struggle commenced between the old form of the institution and the new growing scientific contents, and the attempt to clamp the scientific staff into the fetters of a government civil service consisting of clerical people, having to clock in and clock out so many hours. The intellectual output of the scientific men was measured or determined by the

number of hours they spent there.

DR. KELLOGG: I think our group understands that.

DR. LEVY: Quite. Quite. You mean they appreciate it. Well, those were the early days, but that has been transformed now. The force of circumstance transformed it, and the men who resisted have grown up to take the place of the old ones. So you get this mellowing from the inside. It may happen suddenly. Changes of that kind may be either evolutionary or revolutionary, you see. If you do it intelligently it is effective. If you do it unintelligently it bursts. So we are arguing for intelligence, and I believe you can do it that way. There is always graft and people who are playing for their own hands, but you must always expect that in a community in which people are all different one from the other.

DR. KELLOGG: Possibly one of the troubles with science--and I don't know how we can do anything about it--is that we are always talking to the public about something that has already been done. We are always trying to sell the citizens an old horse. Would it be possible for scientists, on their own initiative, quite apart from political leaders, to look forward a little more in the future and talk about what science may do for the public with the idea of selling them a colt instead of an old horse?

DR. LEVY: Of course, that is dangerous ground. I am not objecting to it; I think it is a good thing, but one has to face the dangers. There are two dangers. One is that you lay out a sort of Utopia and say, "This is the kind of world we shall live in in the future." But people say, "How do you get there?" Hogben does it in his book, you see. As a scientific economist he tells you what science might do. But the important thing is to get out of the present mess. I mean we want the pathway from here to there, which is just as important as the "there." So there is danger of building up a Wellsian Utopia and not know how to get there, and that diverts the energy of the people from the actual activity of getting on to it. I call that a calamity. It is a form of escapism.

DR. KELLOGG: I think that is absolutely right, and in a democracy I would put more stress on the way to get there than on the end.

DR. LEVY: Yes, we are suffering from that very thing in England where the stress is severe, a large number of writers showing ways of escape. They don't do it deliberately.

DR. KELLOGG: That is the big danger, as I see it, in the so-called static planning, to look at it as a bunch of blue prints; you get into that difficulty every time.

DR. LEVY: That is a terrible danger. The thing to overcome that now is to make a survey of the kinds of activities in the community that don't involve so much judgment as scientific knowledge. I am making a distinction

between a knowledge of scientific facts and the importance of the judgment of scientific facts. They both involve ethical, moral, religious, and political considerations. Even in scientific work you cannot help but make a selection of the facts. The individual who runs a public utilities company is primarily an expert. He is a man who is skilled in the scientific administration of a job that has a particular social objective. Of course, we want an intellectual, cultured expert of the type we have been talking about, but there is no moral consideration necessarily involved in it. We hope he is a moral man that won't escape with the funds, but at any rate the moral and political considerations have nothing to do with the question does he or does he not understand his job? You select the man who understands his job, and he is free of all political interference. It seems to me that that is the thing the scientific men must stand for. They must label the jobs in a community that require technical people, and I am using "technical" in a very wide sense. I say it is a dangerous subject, because I have found to my surprise--I didn't know this before--that many of the jobs that we lay down in England as technical are political jobs in the United States. I didn't appreciate that. I may be wrong. Correct me if I am wrong. Take, for instance, running a post office in a town, which seems to me a problem in administrative and technical knowledge in a wide sense; I find that some postmasters here come in and go out when the political situation changes. What if every time the political situation changed the mathematician who was employed in the department had to lose his job and another mathematician had to come in? We just don't have that view. I want to describe the difference between two democracies, that is all, not saying one is better than the other; but it does seem to me that there must ultimately be a convergence of all democracies in the sense that each democracy will eventually designate those particular things within it that are specifically skilled technical jobs and not determined by political considerations. By some means or other any political tinges, either in their economic basis or in the way they are applied, must be exposed and made clear. By the force of public opinion and by the force of scientific clarity it should be finally established that such and such posts are no longer at the beck and call of political chiefs. I ought not to say that. I am just giving you what is the impression that I have acquired.

DR. BELL: I was interested in your discussion yesterday of an interplay between science and society, in which society may forge ahead a little way and bring up society a little bit, whereupon society gets into a war which exerts a suppressing influence. That keeps going on generation after generation. The thing I am wondering about is whether as a result of your study of the history of science and of society there has been some progress made as a result of this constant advance in science in spite of the ups and downs. Are we going to a higher level or are we headed down and out?

DR. LEVY: The very fact you can ask that question is an indication. I remember as a small boy I used to go to a religious class, and I had a brother who was rather irreligious, and he used to put me up to asking questi

of the teacher, and one of the questions he persuaded me to ask was "Could God make a stone so big that He couldn't lift it?" which was a very clever question. Either He couldn't make the stone, in which case He was limited, or He couldn't lift it, in which case He again was limited. I, in my innocence, if you could call it innocence, asked the question, but the teacher knew exactly where it came from. He said, "You go and tell your brother if God can make a boy who can ask a question like that, he can do anything." All I am saying is that if you can ask a question like that it implies we have reached a point of clarity in the understanding of history and historical processes that hadn't been acquired by previous generations. It has only been a short time, indeed, that we have been able to turn down the film of history, as it were, and see the successive pages of history through which people have come, and see a pattern at all.

Most of us were brought up in school with the idea that history was made by great men, just as we were taught that science was made by great men. Of course it was, every bit of it, and the great men always worked within a certain framework, social and economic background, out of which they drew their nourishment, but the tremendous advance was made by the small men. The great man stands on the shoulders of an enormous number of small men, and so on; the whole thing is interlocked in that way. And the history of science, like general history, is a thing with a pattern in it. The fact that we have reached the stage of seeing that pattern means we have reached a new level, and that has come out of the industrial revolution, because having seen the ordinary pattern of physical nature and having a certain pattern in the evolution of men from lower stages, we are now at the stage of seeing the interconnection between the two. The greater the understanding, the greater the capacity to control, and I believe that democracy is the kind of social institution that ultimately can achieve full control of its own future.

DR. BLAISDELL: What you have just said bears on a question that has been forming in my mind for the last few minutes. As a scientist, would you care to speak on this question: what does the future hold in the way of international organization in the light of the kind of world that science has produced?

DR. LEVY: I think we are passing from the stage of having created a democracy or the beginnings of a democracy here, there, and everywhere in different parts of the world. The whole of Europe and the world is struggling to throw off the terms of the old. It may take a long time, it may take 20 years, but we are boiling through that stage, and out of that will come a new organization of society in which the old knowledge of the people will be used to the fullest extent. I see it all over the country and in England. We are the people who are trying to clarify our minds and see the direction in which that change can come. It is actually happening in the community. As the boiling is taking place, there are minds concentrating on the question of how to direct the course of events; but how can it be directed in the middle of the boiling? At the last minute we suddenly wake up to the fact

that the world has fallen about our ears. If we had thought about it earlier, we could have done something about it. To me it is amazing on a morning like this to discuss a question like that. I remember in 1919 writing articles in English journals begging scientific men to desist from the application of science to the indiscriminate production of weapons of warfare without any consideration of how they were going to be used. There were other people besides myself, and it has taken 20 years; after 20 years an enormous number of people are alive to it, but it is a critical situation now, and all we can say is, "The course of history has never run smoothly. It has boiled up periodically. You of the United States I think can learn tremendously today from the lessons of Europe; and just as the English and French democracies are learning rather late from the lessons of central Europe, so the United States can learn from England the lesson which she has learned so late. It takes a longer time for a wave to cross the Atlantic than it does to cross the North Sea, and during the time the earthquake wave is moving, the United States can get ready for the shock. You have to hurry.

DR. BELL: Would you be willing to indicate some of the lines in which you think we can learn from the European situation?

DR. LEVY: These are rather political, I am afraid.

DR. BELL: You may not want to go into it.

DR. LEVY: But I think it is important, apart from the political side of it. It is important to recognize the necessity for scientific men to share their knowledge and understanding with the non-scientific members of the community. I don't mean to popularize science. I mean branching out into the problems that are important to non-scientific people, and bringing scientific methodology to bear on it.* In the process they change their own methodology they change their own method of approach. That unification is absolutely essential, and it is essential to break down the traditional objection to scientific men doing that. When we began to do that in England 20 years ago, al the scientific men got very sniffy about it and said, "Well, he is doing politics," or, "He is doing journalism, or philosoply; why doesn't he just keep to his science?" We must break that down.

Theory and practice must go hand in hand. You may want to get back to the laboratory, but if you are talking about science and society you are talking about science in society, not in the classroom or in the study. That is static. And that is the lesson I think that some of us have learned in England but have been unable to apply. We have attempted to get university departments to set up departments of scientific affairs, philosophy, sociolog of science, a methodology of science, history of science, but that has been quite impossible in England. I have been struggling with that for years. We can't get the money. Of course, the situation is too tense. We can't get

* Compare with the last page of the Morris seminar, p. 195.

people to appreciate its importance properly. But you can see its long range importance and you can build them up. For heaven's sake build them up while the going is good.

DR. SEEGER: I have another question along this line of methodology. I gathered from what you said yesterday that the scientific method would probably take on different colors in different fields. Is there not a danger in our taking a method that is not fully developed at the present time and making applications of it?

DR. LEVY: No.

DR. SEEGER: No?

DR. LEVY: Because if people had always taken that line, we should have had no science. How did we get our science? Men made applications of a method that was not fully developed. Men made discoveries and afterward developed the method. You have to go and do something in order to discover that what you were trying to do was wrong, but when you have discovered that, you have discovered something. That is the positive side of it. If you discovered also that the things you were asking before were rather a foolish question, then next time you wouldn't ask that kind of question.

Every scientific experiment involves an addition to the scientific method. In exactly the same way, when we come out of the laboratory and take part in social affairs we shall discover the weakness of the ordinary processes of physical science. The process is one in which you stand here and the experiment is there, and you may have no effect on the experiment, having set it going, turned on the knob if you like, and it goes through its process. If you are not quite certain it is right, you set it up, you turn the knob again, and it repeats. This capacity for repetition is an essential part of a physical experiment, but where did you ever see anything repeat in nature? What we are discussing when we are dealing with social affairs is a changing pattern of a non-repetitive nature. There is something underneath it deeper still which you can say is repetitive. For instance, successive stages of understanding are repetitive but the different stages are different. This morning's talk has changed us all, you and me, we are different for the experience. We couldn't all go out and say, "Let's start again now." We can't do this experiment again. A social affair is not a thing that is repetitive like a science experiment. You would know from your own experience that people are different from an experience. A machine is not different from an experience--not to any significant extent--so it can repeat.

DR. SEEGER: Are there certain definite laws of change relating to human actions?

DR. LEVY: Yes, there must be certain definite laws of change. Take, for instance, London on Saturday afternnons. There are thousands of people--

at least 200,000 people--who go to football matches. There are three or four large football grounds, and if you were in an airplane looking down on London you would see the mass movements of people going out to the football grounds--if you could see them. Actually they go by underground, by subway. The railroad company knows roughly how many people are going to go to each particular place, when they are going and when they are coming back. So here is a social regularity on a Saturday afternoon in London. It is a social custom, if you like, and it is a numerical one. You can measure it in the ordinary way in which you can measure a statistical phenomenon. To cope with that regularity, the underground railroad company and the bus company runs a regular service which it speeds up and increases in number just at the critical points and critical times. You see the regularity among human beings--a social regularity--being built up by the social organization and organized institution of the community. These two are linked together, one reflecting itself in the other. I could take any one of the people going to the football match and I could say, "How did you come to make up your mind to go to the football match?" and he will give me a tremendously complicated explanation of how he decided to go, a struggle of his free will with this, that, and the next thing, and finally the real outcome was he went to the football match. But the railroad company is not interested in them as individuals. Out of the struggle of the mental decisions of these people as individuals there emerges a regularity, and the railroad company would be entitled to say, "Here are all our people exercising their free will. Here is the law of the free will in the mass." It is a unification between individual freedom and group regularity; group regularity showing itself in individual activity. These are laws of group behavior, and there must be processes of change that go on.

DR. BLAISDELL: Would you go so far as to say that those laws of group behavior are in any way comparable to the so-called laws of physical nature?

DR. LEVY: Yes, I think there are certain similarities and certain dissimilarities. For example, there are definite laws describing the drift of the stars. These are regularities in the heavens. Individual planets, and so on, are moving about in all sorts of higgledy-piggledy ways, but there are certain regularities that can be discerned, and the same thing applies to these human beings. But on the other hand we have to remember that these human beings are conscious beings. They are not pieces of matter. They are not pieces of material that are flying about. So you get laws that are specifically for inert matter, and you also get different kinds of laws--we call them laws--descriptions of behavior for conscious matter. You can see the difference quite clearly. Newton will describe the law of behavior of a particle shot into space and make a prediction accordingly; but if I say to you: "What is so-and-so like? What sort of fellow is so-and-so?" You say, "Oh, well, he is the kind of man that if he borrows a dollar from you, he doesn't pay it back." That is a regularity he has. That is his regular form of behavior. You see, he is really telling you something about a characteristic that shows itself in a varying environment or a specified environment. He has such and such a characteristic. It is the beginning of the laws of a living individual. But of course he is much more complex than

that, because you might say, "Yes, also he is the kind of a fellow that if he hears me saying he doesn't pay the dollar back, he pays you back." So he is a little more complex, you see. When we are talking about a conscious particle, we are talking about something that still has regularities in it, but much more complex than the material particle I was talking about. Groups also have regularities. Insurance companies use them.

MRS. MILLER: What do you think of the methodology that you mentioned yesterday—a new committee that the British Association has set up, whose purpose is specifically the relationship of science to some of the more social aspects of affairs? Do you think that is preferable to a person-to-person approach? It is not an either-or affair, but where do you think the emphasis is more likely to be conducted?

DR. LEVY: To begin with, I agree with you, it is not an either-or affair. They can go along side by side, except that people having limited energy can't do both to the fullest extent, but my view is we must see human change at its different levels of law behavior. There are individual laws which I mentioned a moment ago, which would, when put into operation, involve the personal relationship, by the people passing on the knowledge of propaganda, call it if you like, from one person to the other. Then there is the group activity which then lays down the action within which the individual is conditioned and brought up, and therefore I believe it is better to do it in terms of the group laws and the conscious direction of group laws. It is better to do it by bringing organizations into being which consciously are planning to spread this kind of knowledge throughout the larger section of the community. It is part of the process of education. There is the thing called P. E. P. in England--Political and Economic Planning, which collects data and attempts a certain amount of interpretation. These people are represented under a new division of the British Association. There are other study circles and groups of various kinds doing specific jobs for the new division, and they are taking it in earnest.

DR. BLAISDELL: Dr. Levy, is science absolutely purposeless as you inferred yesterday without actually saying so?

DR. LEVY: I don't say it is purposeless. I say my view is that if you take a film of history and you put human beings on it from the early state up to the present date and see the various stages of society and the kind of science produced at each stage, you can see what the historic purpose was at each stage. It was to create the next stage. There were forces at work in each stage transforming into the next, transforming the feudal system into the mercantile system, transforming the mercantile system with its craftsmen into the industrial system. You can see that the activity of the people in the feudal age was to create the knowledge and understanding required to bring into creation the next level of social development.

DR. MILLER: Yesterday when you referred to the disappearance of the feudal system and the growth of the industrial system it sounded to me as though you tied it up with the growth of democracy. Was that a correct in-

terpretation?

DR. LEVY: Yes, I think that democracy has necessarily grown out of the activities of the industrial system.

DR. MILLER: I tell you why I am asking that question. Some of us who are somewhat superficial students of history are rather inclined to agree with Bryce's prophecy in regard to democracy in this country. That was that as the rural communities lost power and the large cities gained power, and these corrupt gangs came up, pure democracy would suffer.

DR. LEVY: Well, pure democracy is an abstraction. My idea is that in creating a democracy human beings are creating a way of living in which they can control their own future in the most effective way, with an understanding of what they want to produce, what they want to create, and you can see the gradual process by which people have come to acquire that understanding. If we look at the growth of the democracies in the 19th century, we run away with the idea; at least in England we have the idea that the English democracy is democracy and there is something wrong with the American democracy, and then we come across here and people here think American democracy is better.

DR. MILLER: We may both be right.

DR. LEVY: Of course, they are both different aspects of the same general thing, and French democracy, too; but there are others--Russian democracy. A Russian worker told me on one occasion when I was there in 1934, "We have a meeting every month in which we discuss the running of this factory. Every worker in the factory that has anything sensible to say has a right to get up and say it. There is no nonsense about it. He has equal right with anyone else. If he talks nonsense, he is shouted down, but if he talks common sense it is discussed." He said, "Can you do that in England?" I said, "No, the industry is owned by somebody who doesn't like that kind of criticism. He knows best." He said, "You call that a democracy?" I said, "Well, we can do other things you can't do." He immediately put me on the defensive. He did have a freedom that was absent in my country. We had become so accustomed to the absence of it that we didn't feel it as a restriction. Here are two different kinds of democracy-- that is all. Here are people living in two different economic social environments in which they are both working out thei own salvation in their own way. I believe in the end they will all converge because people have to find the same way of living as human beings. They may be starting from different points of the compass and coming towards the centre in different ways. We must view the democracies as being different forms of democracy in different countries. British democracy and American democracy began at the beginning of the industrial revolution. It began when it became necessary to use people to do machine work. I saw it clearly when I was in Russia. There you had a population that was largely illiterate and here they were trying to introduce a machine age in factories in a country that had no factories. They couldn't read blue prints, couldn't read and write. There were no books. They wanted people to make machinery, and they couldn't count,

they couldn't measure. In order to train them to do these things they had to teach them to read, write, do arithmetic, do mathematics, read blue prints, design this and that, had to educate them in the modern sense. You can't produce a machine age without producing an educated democracy because educated people ask rights. They may not get them yet, but as sure as fate, make people intelligent and understanding and they ask for freedom. That is part of the nature of human beings. They are asking for the right to expand and control some of the things we are discussing now. So in the early 19th century when the early machinery began to be devised out of the work of the craftsmen, production became greater. You had developed a population which had to work machines. We brought them together, a dozen of them, instead of each one doing his own job. Each man was talking to the man next to him on the bench instead of wandering in the fields week on end never seeing a soul, having no conversation of any kind. You developed a different people and these people having come out of that are the democracy. A technological age created a democracy. It gave them a power over nature which they demanded they should exercise, and what we are witnessing today is the coming of that technological age, creating on the one hand control over natural materials, and on the other hand a people who are asking to be able to do it, and the function of science is to unify these two. That is all. So we will get a democracy controlling its own future.